JOURNAL

OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

PUBLISHED UNDER THE SUPERVISION OF THE EDITING COMMITTEE,

AND EDITED BY

F. H. WEBB, SECRETARY.

VOL. XXIV.—1895.

London:

E. AND F. N. SPON, 125, STRAND, W.C.

New York:

12, CORTLANDT STREET.

1896.

TABLE OF CONTENTS.

VOL. XXIV.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXIV. 1895. No. 115.

The Two Hundred and Seventieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 10th, 1895—Mr. ALEXANDER SIEMENS, President, in the Chair.

The minutes of the Annual General Meeting, held on December 13th, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates—

Henry Ridley Burnett.
William R. Cooper.
Percy Walcott Dowse.
Charles V. Drysdale.
John Fowler.
Francis Harrison.
David King Morris.
W. M. Morrison.
W. Prout Whitehead.

Mr. Charles Bright, Member, and Mr. H. E. Mitchell, Associate, were appointed scrutineers of the ballot for new members.

The PRESIDENT: I have to announce that Mr. A. Calder has resigned his position as Associate Member of Council, and that, under the authority vested in them by No. 42 of the Articles of Association, the Council have elected Mr. J. Ardron in his place.

I now have to present the premiums awarded in respect of the session 1893-94, and I will first call upon Mr. H. D. Wilkinson to accept the instruments and books, which he has chosen, representing the Institution Premium, value £10, awarded to him for his paper, "Notes on Electric Tramways in the United "States and Canada."

The Students' Premium, value £3 3s., consisting of a planimeter, has been awarded to Mr. G. C. Allingham for his paper, "On Secondary Batteries," and I have much pleasure in now presenting it to him.

The "Salomons Scholarship" having been awarded to Mr. Ernest Hoadley, a student at the Finsbury Technical College, I have great pleasure in handing him this cheque, £50.

This pleasing duty is the last which I have to perform as your President; and as no doubt you are all anxious to hear Mr. Crompton's Address, I will not say anything more this evening, except that, although during the year I have occasionally been prevented, from causes beyond my control, from attending in my place either here or at the Council table, I have, nevertheless, had the hearty co-operation of my colleagues and your kind support, which has rendered my term of office at least very pleasant for me. For that I thank you very much. I have now great pleasure in introducing Mr. Crompton to you as your new President. "Good wine needs no bush;" so there is no occasion for me to allude to his eminent qualifications for the office to which you have elected him.

Mr. R. E. Crompton then took the chair as President.

Professor W. E. AYRTON, F.R.S.: The Secretary has just placed a proposal in my hands which I shall be very happy to bring before you: it is the proposal of a vote of thanks to our retiring President, Mr. Siemens. Mr. Siemens has said that he has not attended as often as he might have done. That may be his view, but it certainly is not ours, because we think that he has attended as often

as it was possible for him to do. It is a very nice thing, I dare-say you think, to be President of this Institution,—a great honour, no doubt; but, like many other honours, there are certain onerous duties connected with it.

If a President had merely to come to the general meetings here, his life would be one of continued bliss; but—and it is a very great "but"—there are very many committees in connection with this Institution. There is the Finance Committee, the Editing Committee, the Technical Committee, and, every year, Members of Council and other members of the Institution suggest that there should be other special committees. Of course it could scarcely be expected that a busy man like Mr. Siemens should attend all these committee meetings, but he has done so. Sometimes, in fact, he has composed the whole meeting himself, for nobody else besides the President and the Secretary have been present. However, it is an old adage that the busiest men find most leisure, and the adage has certainly been proved true this last year. Mr. Siemens—the moving spirit of the large firm of Siemens Bros., occupied in carrying out work all over the world—has found time, not only to attend most of the Council meetings and the general meetings in this room, but to attend all the committee meetings. I have, therefore, the greatest pleasure in proposing that the cordial thanks of the Institution be given to Mr. Alexander Siemens for the admirable manner in which he has discharged the duties of President during the past year.

Sir David Salomons: I have very great pleasure in seconding this vote of thanks to our late President. As Professor Ayrton has told you, Mr. Siemens has attended to his chief duties—those which interest the Institution far more than taking the chair in this room—that is, looking after your business in the Committee Room and in the Council Chamber. As you know, this gentle-man has served with courage in the field 24 years ago, and received the Iron Cross medal for his services. We are glad to see that he escaped to become a naturalised Englishman, to be one of us. He has raised himself not only to be the head of perhaps the greatest firm of electrical engineering in the

world, but he has also raised himself to be the President of this Institution, and we have been proud to see him occupying that chair. I have very great pleasure in seconding the resolution, and let us hope, although now a "Past-President," he may for a great many years remain with us.

The PRESIDENT: It is my first and most pleasing duty to put this vote to the members, and I hope you will show your acceptance of it in the customary manner.

The resolution was carried by acclamation.

Mr. SIEMENS: Gentlemen,—I am afraid after all this praise which has been bestowed upon me I should be winding up in a very bad style if I kept you waiting any longer for the Address of Mr. Crompton. I am extremely glad to think that you have appreciated my efforts to do my best for the Institution of Electrical Engineers, and I shall certainly strive to continue to do so in future.

The PRESIDENT then delivered his Address.

INAUGURAL ADDRESS.

By R. E. CROMPTON, M.Inst. C.E., President.

I think that I shall interest you most if I address you on electrical engineering from my own standpoint. I believe that I am the first of your Presidents who received his early training in mechanical and structural work entirely unconnected with electrical engineering, which at that time meant telegraph engineering. You must, therefore, not be disappointed if I deal with electrical engineering chiefly from the mechanical engineer's point of view, believing as I do that its great development dates from the time when the mechanical engineer first took in hand the design and construction of dynamo-electric machinery. This date is that of the Paris Exhibition of 1878, so that we have had 16 years in which to develop the dynamo, the alternator, the transformer, and the storage battery into their present form. During these 16 years the dynamo, in mere size, has

increased 500-fold, its efficiency has increased from 60 to 97 per cent.; from an instrument maker's model it has become a machine which, whether as regards solid mechanical construction or smooth efficient working, will compete favourably with any form of engine or machine tool that has ever left the hands of the mechanical constructor. The modern alternator has been slowly evolved from the original Alliance magneto-electric machine, the transformer from the induction coil, the storage battery from the original Planté cell. When I say that the vast development which has taken place during these 16 years has been mainly due to the labours of the mechanical engineer, I do not in the least undervalue the immense labour of the physicists and of the telegraph engineers who preceded us; on the contrary, it is abundantly evident that this rapid development was only rendered possible by the splendid work which the physicists had already done, laying down so clearly the laws and the co-relation of electro-motive forces, electric currents, resistance and induction, and to the admirable system of units which, through the British Association committee, the telegraph engineers had worked out to such completeness, that they now form with very slight changes, the complete system of modern units that are universally adopted, and I cannot omit to mention the great impulse which was given to the progress of electrical engineering by the invention of the incandescent lamp by Swan, Stearn, and others. For it is quite certain that, although the development of dynamo plant for arc lighting and for transmission of power would have attained considerable proportions without the assistance which it has derived from the demand for incandescent lighting, yet it would have taken a vastly longer period to have reached its present stage of development.

I am one of those who, when I use the word "electricity," have before me a mental picture of an agency which we use for producing action at a distance, an infinitely flexible connecting-rod by which we can transmit energy and reproduce it at a distance in any desired form. Perhaps the reason why I have formed this mental picture is that I entered on the study of electrical engineering through a very correct process of evolution.

Having been previously engaged on a practical application of hydraulic transmission plant, I was, from the time that I first commenced on electrical work, struck by the general resemblance which the flow of electric currents through conductors bears to that of water or other fluids through a system of pipes. It is almost a pity that the actual demand for electrical energy for light-giving purposes preceded that for mechanical power purposes, as the part which electricity plays as a connecting-rod only is not so immediately evident when it is reproduced as heat for electric lighting as when it is reproduced as mechanical energy in a motor.

At a very early stage in the mechanical development of electrical machinery, the mechanical engineer found himself confronted with an entirely new set of problems; he found that the laws which govern the flow of the electric current and the formulæ he had to use were simplicity itself compared with those he had been compelled to master before he could deal with the design of any form of thermal engine, or even deal with the flow of fluids or liquids through pipes, the chief reason being that the electric current is so easily confined—"canalised," as the French call it—in its conductor by its insulation that the losses due to electrical leakage may in most cases be neglected, whereas the losses in thermal engines through the walls of the passages or confining chambers are so great, and form such a large percentage of the whole of the energy, and vary through such very wide limits according to the form of these passages and to the materials of which they are constructed, that the calculations involved in any new design are extremely complex. On the other hand, the mechanical engineer found that the necessity for so frequently interposing insulating materials between the various parts of electrical machinery greatly increased the difficulties of mechanical construction. Nearly all insulating materials are deficient in mechanical strength; in most cases they do not maintain their form through any considerable range of temperature, are readily damaged by oil or grease, and do not readily lend themselves to being cut or shaped by ordinary tools.

Constructors of electric apparatus have to deal chiefly with

iron, copper, and insulating materials. The improvement in the electric qualities of the first two have been very marked, and we have probably nearly arrived at finality, but we have made but little advance in the improvement of our insulating materials. Probably of all the materials that are available for insulating or isolating the parts of generating machinery, transformers, and distribution appliances, mica is the only one that at all approaches our requirements, and mica is only satisfactory when it can be applied in thin sheets to insulate adjacent surfaces of considerable area. Slate, porcelain, and enamel, when used as insulators, although they can stand considerable changes of temperature without alteration of their insulating qualities, are deficient in mechanical strength, and cannot be readily shaped or cut. Wood, hard rubber, and the various patent materials—such as woodite, vulcanised fibre—although they are more satisfactory as regards mechanical strength, are considerably affected by heat, moisture, and oil or grease. The various slates and marbles are mechanically weak, and, unless protected by enamel, are absorbent, and partially lose their insulating qualities in a damp atmosphere. Attempts that have been made to mould or cast insulating materials out of mica waste or similar substances, cemented together by shellac, resin, or other adhesive materials, have only obtained partial success. In no case has the double requirement of mechanical strength been combined with that of resisting change of temperature or the absorption of moisture. This difficulty in obtaining satisfactory insulating materials has been the chief stumbling-block which makes the work of the designer of electrical machinery so much more difficult than that of the designer of ordinary metal work, where the necessity for insulating the parts does not come in.

The improvement of the material that must be used for insulating the distribution system of conductors has been an equally difficult matter. The study of this question was forced upon us as soon as it became evident that electrical energy could be commercially supplied and distributed throughout our streets. The problem has been attacked in various ways. The

various systems of insulation that have been tried can only be tested by time; it will be many years before we shall be able to predict with any certainty as to which systems are likely to survive. It is impossible for me to devote more than a few lines to this interesting question. As you know, the systems of conductors that have been hitherto laid divide themselves roughly into built-in systems, drawn-in systems, and those which use bare conductors insulated by glass or porcelain. Quite recently the attention, not only of ourselves, but of the public, has been directed by several accidents to the dangers attached to electrical conductors laid in the streets. In most cases these dangers are not properly attributable to the electric conductors themselves, but to the presence of gas in the soil through which these conductors pass. Wherever any space exists into which this gas can infiltrate, an explosive mixture may be formed, which may be fired either by an electric spark from the conductors themselves or by contact with a match dropped by a smoker. I think, however, that there is no reason for the public to unnecessarily alarm themselves. In the vast majority of cases where the system of conductors is continuously inspected for traces of gas no accidents of any kind have occurred.

Although I have already stated that electrical engineering is greatly indebted to the mechanical engineer for much of the development that has taken place in recent years, yet in some respects the reverse is the case. The alliance between us has not been wholly productive of benefit to us. An electrical engineer nowadays has to grasp the whole subject of the production and distribution of electrical energy, from the energy contained in the coal to the point at which it is delivered to the consumer. As regards a very large portion of the plant he has to use, the electrical engineer has had to adopt the appliances already sanctioned by the mechanical engineer: I allude to the boilers, steam engines, and to the motive power generally. When the electrical engineer called for a motive power on a large scale, he found ready to his hand the experience of those who had equipped the great factories of the manufacturing districts of England; and these gentlemen have had a considerable hand in

the design and application of the motive power we use, and I am afraid that in many cases the combination has not been altogether a paying one. Previously to the introduction of power houses for the generation of electrical energy, the large mill-owners were the principal users of power in England, and it is to their requirements we owe the developments of the Lancashire boiler and the refinements of the large slow-moving engines that are always found in such factories. These gentlemen are perpetually reminding us that in order to obtain the maximum of economical success we have only to copy their practice, and that anything that differs from their practice is wrong, and I am afraid a very considerable number of electrical engineers, particularly the younger members of our profession, have been considerably swayed by these utterances. I feel very strongly, however, that it is time to speak on these matters with no uncertain voice. The conditions under which we work our engines, boilers, and other motive-power plant for electrical supply purposes differ so widely from the conditions of motive power for driving factories that their experience is of little or no value to us—in fact, it is in many respects misleading; and I take it to the credit of electrical engineers that it is we who have been mainly instrumental in developing the newer types of engines and boiler plant which are now largely used in our supply stations. I think that few will doubt it was the exigencies of electrical engineering that led the late Mr. Willans to carry out his extensive series of experiments on the economical use of steam which has so revolutionised our ideas on the subject. If it had not been for the conveniences for accurate measurements which the dynamo and electric measuring instruments offered, Mr. Willans's experiments could never have been carried out to the convincing degree of accuracy which he attained. I think, from this and from other causes, that we electrical engineers are already beginning to return to the mechanical engineers some of the benefits that we received from them. I am sure that the considerations of load-factor and all similar matters, which have been found of great importance to electric supply engineers, will be found to have almost equal importance in the matter of waterworks, gasworks, and many other industries. In a word, I believe

that the studies which we have been compelled to pursue in order to investigate and deal with our electrical difficulties will prove to be of considerable service to the engineering world in general, and that the engineering world in general will gradually become more and more intimately connected, so that eventually some part of the training of every engineer who deals with constructive works or supply works of any form will be an electrical one.

Not the lightest of the duties of the modern electrical engineer is that of educating the public in the use of electrical energy. This process, which we in this room call the improvement of the load-factor, is a matter of such importance to you all that I need not apologise for dealing with it at some length. We not only desire to have the output of our generating station more evenly distributed throughout the 24-hour day, so as to fill up the valleys and reduce the peaks of our daily diagram, but we also wish to improve the summer diagram as compared with the winter one. The problem of satisfactorily and economically working the daily diagram may be, to some extent, dealt with by improvements in the storage of electrical energy; but, even if we were in possession of as satisfactory a system of electrical storage as the gas companies possess in their gasometers, it would only help us to this point. The great disparity between our winter and summer loads is due to our geographical position, and will always exist so long as the major part of the energy we supply is for lighting purposes, and the obvious remedy is to encourage its use for motive power and for heating and cooking; and to me at the present time it appears a far easier matter to educate the public to the advantages of using electricity for heating than to induce them to use motive power to any considerable extent. For this reason I have persistently advocated the perfecting of electric heating and cooking appliances. This matter has not been much understood even amongst electrical engineers themselves. For a long time the opinion was very generally held that the energy required for electric heating is so great that its cost would be prohibitive. At first it was not seen how effectively electrically heated appliances could be used for cooking purposes, how large a proportion of the heat units would be usefully

employed in cooking food, and how small a percentage would be wasted in heating surrounding air. Now that this result has been obtained, and that cooking operations on a considerable scale can be carried on in an ordinary sitting room without perceptibly raising the temperature of the air of the room, it will be seen that electric cooking is certain to obtain popularity in hot climates, or in summer, or in small tenements at such time when the lighting of fires and the waste heat from these fires is not only unnecessary but actually objectionable, and this even if the cost of the electrical cooking be considerably in excess of that by other means. At any rate, we may assume, if these expectations are only partially realised, that there will always be some sale of electricity for these purposes, and that the addition to our load diagram from such use will take the form of adding to it a parallel strip commencing from 7 o'clock in the morning and ending at 11 o'clock at night, as I do not think there is anything in the condition of electric cooking to warrant anyone in thinking that the demand for energy for this purpose will be confined to the period immediately preceding each meal-time. The use of electricity for heating will be probably confined to the partial heating of rooms or parts of rooms which are required to be occupied for short periods or for the airing or warming of clothes or linen, and such use is also likely to be confined to the same hours as I have given for cooking.

Turning now to the possible extension of the load from the use of motive power, we know how large a portion of the income of the American supply companies has been during the past two years obtained from its motor load, and that up to the present our motor load in England, at all events in London, has been insignificant. I have attempted to find out, by putting questions to the managers of the most important American electrical supply works, whether the habits of the American people, or the construction of the dwellings in which they live or of their business premises, was such as to cause a special demand for motive power which would never arise on this side. In putting these questions, I fully expected to hear that their large motor load would be chiefly attributed to the driving of elevators, to

the ventilating of their rooms during their exceptionally hot summers, and to the working of special machinery necessitated by their lofty buildings; but I am told that this is not so, that many of the large hotels and larger blocks of buildings possess their own separate plant for working their elevators and other machinery, and that the bulk of the motor load of the electric supply stations is obtained from the buildings of a moderate size, and that in these the motors are used for small industries and for domestic work, both of which uses are likely to exist to an equal extent in London or in any large English town. If this is the case, we may feel sure that the commercial enterprise of those amongst you who make and sell electric motors and the engineers of our supply companies who are so desirous of increasing their motor load will be sufficient to make it gradually approximate to the American one. In this connection it is curious to note that, although we, in our central stations, are so anxious to increase the output of our plant, and hence our load-factor, by encouraging the use of electric motors, the main argument we must use to an intending user is that of all sources of motive power none can be worked with so little waste of power during long periods of idleness as the electric motor. In other words, the electric motor is the cheapest of all systems of driving machinery when its load factor is at its lowest. To mention a very ordinary case. Throughout all residential districts there exist a considerable number of small shops for repairing and in some cases making domestic furniture on a small scale. In many cases it would be an advantage in such a small shop if one or two machine tools, such as a saw and a moulding machine, could be occasionally used for preparing the material. Such a machine would require from 3 to 5 H.P. to drive it when doing useful work. In most cases these machines would be only used for a few minutes at a time, several times a day, the total use probably not extending to more than seven hours per week. If the user obtained his power from a central source of supply, and paid 4d. per unit for it, the total cost of working these two machines would only be about 7s. per week, although the above time would be made up of many

short periods of use. If we add on an ample sum for the interest on the cost of the machines and motors and for their maintenance, the total cost need not exceed £1 per week, so that it is evident that, in many cases, it would pay well to employ such tools and motors, rather than send work out to be executed at a saw-mill. It is evident that if the same intermittent employment of power had to be obtained from a steam-engine or even a gas engine, the total cost of full attendance and upkeep would be from £3 to £2 per week, quite apart from the many inconveniences and dangers attending the use of steam power in small premises. It is evident, however, that as you increase the hours during which these machines are worked, the electric motor will lose its advantages, and a point will be reached at which, first, the gas engine, and eventually the steam engine, will be found to compete favourably as regards cost.

This argument of the cheapness and convenience of the electric motor for very intermittent supply of power is one which cannot be impressed too strongly on likely future users of power; but, apart from what we can do in supplying power to such small industries, there is much to be done in this country in familiarising the minds of the inhabitants of Mayfair and Kensington—to take these districts only as typical of many others—that the use of motive power in private houses need not be limited to working lifts, driving sewing-machines, knife-cleaners, grinding coffee, or even, as has been stated in another Presidential Address, "brushing "their boots, washing their faces, and serving their meals." With great deference to the gentleman whose words I have just quoted, I must submit that there has hitherto been a certain want of imagination shown whenever the question of the possible extended use of motors has been discussed. No one can help noticing that amongst Englishwomen and Englishmen, at any rate, the desire to work at home, to make something useful or artistic, or to decorate or to beautify our homes, is very widely spread. The number of those who employ their otherwise idle fingers during their leisure time in some kind of work is very great, but hitherto this work has been confined to sewing embroidery, etching, painting, light carving—at any rate, to work where the muscular

exertion required is small. This practically excludes any constructive or decorative work which requires to be made out of materials of a hard and enduring nature, but if once the electric motor is pressed into our service, if the electrically driven shaper or graver needs only to be intelligently directed by the user, the most delicate-handed lady could cut and carve out designs in the hardest materials with the same ease and same certainty that she now uses the embroiderer's needle or the etching pen. There is one use to which electrical energy can be applied in very considerable quantities in residential districts. I allude to the production of artificial cold by means of electric motors. You all understand that this is at present done by arranging them to drive an air compressor, and afterwards expanding the compressed and already cooled air into any properly constructed cold room in which it is desired to maintain a temperature sufficiently reduced to store perishable eatables—such as meat, fish, game, and vegetables or fruit—for long periods. Such cold storage would enable each consumer to deal far more directly with the producer than has hitherto been the case; the cost of several daily deliveries of food articles would be practically abolished, and the extravagances and waste of housekeeping would be reduced to the lowest possible limit; in fact, the advantages of such cold storage are so obvious that it is unnecessary to further insist upon them.

There is another development of the use of electricity for motive power to which I think your attention should be directed. The President of the Institution of Mechanical Engineers, in his address to the mechanical engineers in April of last year, drew attention to the efficiency that we claim for electric transmission of power to machine tools, and compared it with the efficiency of existing systems. I have Professor Kennedy's permission to use his address as a means of carrying this highly interesting problem a step further. He said that, "in any given factory "running on the ordinary system there is a large continuous "waste of power due to the running of the whole shafting, no "matter how many or how few are at work; under such con- "ditions, the waste work in shafts and belts may well be 25 per

" cent. of the average useful work, and the distribution of total " work may be approximately—

" For the useful work	100
" Wasted in belts	25
" Wasted in engine friction	20
	145 "

He compared this with an electric transmission as follows :—

Useful work	100
Wasted in motors and dynamos ...	24
Wasted in leads	2
Wasted in engine friction	20
	146

so that he shows that the two sets of figures are practically the same as to amount of power required. I think that the cases where the comparison would work out on such equal terms would be very rare indeed, and would only be likely to apply to textile or similar factories. In engineering shops, where the time during which each machine tool is standing for the work to be fixed is very large compared with the time that the tool is actually running, I think it would be not too much to say that in most such shops the useful hundred horse-power he gives in his first table might be divided by three, the other losses remaining constant, so that the average useful effect throughout the day is not as 100 : 145, as given by him, but rather as 33 : 78; in other words, the average efficiency, instead of being 68½, would be really about 42; but in such a case, if electric transmission, with separate electric motors for each tool, be used, the efficiency can remain at 68 as before. Therefore I believe that the cases where electric transmission of power can be substituted with considerable advantage over present systems are far more numerous than Professor Kennedy's figures would lead us to suppose. It will be noticed that the difference between my assumptions and those of Professor Kennedy lies chiefly in our estimates of the load-factor of each of the tools.

Several times when addressing you here I have used the term "load-factor" to denote the ratio which the total useful time

during which the appliance is worked discontinuously bears to any given period, whether it be an hour, a day, or a year. As you know, I found myself compelled to investigate this question when dealing with the cost of electrical energy in my paper before the Civil Engineers; but this load-factor question has a far wider application. If an attentive observer walks round a factory where machinery of any kind is being used, and takes careful note of the relation of the periods of useful work of each machine to its periods of idleness, he will be struck by the large proportion which these hours of idleness bear to the hours of usefulness. As regards the machines themselves, the only loss caused by these long periods of idleness is that due to the interest on the money invested in them; but in the case of the engines which drive them or of the transmission devices, such as shafting or belting, in addition to the loss of interest, there are continuous losses due to the waste of power and cost of attendance that is required to keep them in motion at the required speed during these idle periods.

This matter is of great importance to us electrical engineers, for of all methods of transmission of energy that now exist, none can be worked with so little waste of power during these periods of idleness as electrical transmission. I have already shown this to be the case where the source of power is obtained from an electric supply station, and it is certainly the same in any works where any considerable number of tools are required to be intermittently driven.

One of the chief difficulties that have been met with in replacing the muscular energy of ourselves or of animals by that of the forces of Nature has been that of providing a variable ratio between the two factors of speed and torque that is necessary if we have to obtain from a constant source of energy the varying duty that we usually demand from our muscles. All our machine tools are provided with a few changes of the speed gear which forms a clumsy approach to this requirement. I have sought in vain for a short expression to define such a variable gearing, but am obliged to content myself with calling it "speed torque "ratio gear." In all attempts to carry this out mechanically the

change of ratio can only extend through very narrow limits, and at the outside four or five steps in the change of ratio can be given. Mechanical engineers for many years have been looking for such a gearing which will vary the ratio exactly as required, and not by steps. The nearest approach to this has been by two reversed cone pulleys with a round belt of constant length which is moved or retained in any desired position by a fork sliding in guides. Quite recently Mr. W. W. Beaumont has introduced a mechanical variable ratio gear which is a decided improvement; but in my view the first real step which promises to revolutionise our ideas on these matters has been the working out of this problem in the electrical portion of our plant—*i.e.*, in availing ourselves of our really flexible connecting-rod, electricity itself.

One arrangement of this kind has been worked out in America by Mr. Ward Leonard. If it is desired to provide from a constant source of energy at the axle of the motor C, either great torque at very low speed, or the same energy in the form of reduced torque at the higher speed, this is done by Mr. Leonard by passing the energy through a motor-generator, A B, the motor, A, of which may be either an electric motor driven from a central station, or a steam, gas, or water engine, but in every case delivering a constant supply of mechanical energy to the spindle of the generator portion, B, the armature of which is connected by short conductors of low resistances to the motor C. If the magnets of motor C are kept supplied with a constant field from a constant source of supply, and the magnets of the generator B receive a variable degree of excitation by inserting a rheostat in series with them, so that the field may be varied through wide limits; by commencing with an extremely weak field in the generator B the motor C can be started with a large current, giving large torque at low E.M.F., then as the excitation of the field of B is gradually increased the E.M.F. of the generator and of the motor will also be increased, and at the same time the torque diminished, so as to get the spindle gradually into motion, and the machine driven by the motor C steadily brought up to its proper speed.

I give the description of this beautiful electrical gear just as it is given to me, and am told that there are no difficulties found in working it in actual practice; but I feel sure that it is only one of many similar methods by which this variable torque and speed ratio can be worked out electrically, and with a simplicity in the moving parts resembling that of the muscular system of the animal world. The application of this invention to working long lines of railway would appear to get over the great difficulty which would be due to the heavy fall of potential which occurs at the times when large currents are taken by the ordinary train motors whenever a train starts from rest, this difficulty being only slightly reduced by the device of motors arranged in series, and afterwards switching them into parallel. With Mr. Leonard's arrangement a long line may be worked at a high E.M.F. of 10,000 volts, generated by monophase alternating machinery, transformed at intervals and supplied to sections of the line at an E.M.F. sufficiently low to be easily dealt with—say 500 volts—but still in the form of an alternating current. This alternating current could be collected from the line and used to work the motor A, as the arrangement I have just described would allow that this motor A should be a monophase synchronising motor, worked at extremely high speed, and having the generator B driven by it; the generator B and the main motor C would be of the continuous-current type, and there is no reason why the motor C should not be of extremely low E.M.F., and consequently easily made direct-acting on the driving axle.

I have described this invention at some length, for, although it may have been noticed by some of you, it has probably escaped the notice of many who are engaged in thinking out the interesting problems of electric traction, haulage, or other uses to which the use of electric power is every day being applied.

We electrical engineers are frequently complimented on the perfection to which we have brought our measuring instruments, and in most cases this praise has been fairly earned, but I think some protest should be made against the very impossible degree of accuracy which is often claimed for electrical measurements. Such claims for accuracy those more thoroughly conversant with

the subject know to be unwarranted; they arise from an incomplete appreciation of the difficulties which attend the making of very accurate measurements of any kind. We often see the efficiencies of the various classes of electrical apparatus worked out to several places of decimals, when it is well known that no instruments exist which could record the measurements to this degree of accuracy. Electrical measuring instruments depend in the majority of cases on the accuracy with which they can indicate the amount of the minute forces to which their moving parts are subjected. In order to make them as sensitive as possible, these moving parts must be delicately adjusted or suspended, and the accuracy of the readings depends entirely upon the degree to which the instrument can be depended on to reproduce exactly the same readings under the same conditions as those observed when it was originally calibrated. It is evident, however, that this accuracy can only be maintained if the friction of the parts and their correct position and the strength of the controlling forces remain exactly the same as they were at the time of calibration. In practice it is found impossible to guard against minute changes in these conditions; so that, however accurately the instrument of precision may be calibrated in Scotland, the very delicacy of its pivoting or suspension, which are necessary for accuracy, make it quite certain to lose its correct calibration after a journey to London, and no one could swear to the correctness of the readings. The case becomes worse if such instruments have to be used as travelling standards of reference. Take the very common instance of an inspecting engineer going to a maker's works to test the efficiency of a steam dynamo, the passing of which may depend on 1 per cent. of efficiency. In order to simplify this matter, I will suppose that there is no dispute as to the measurements of indicated horse-power, but that it is confined to the difference between the electrical readings of the maker's instruments and those of the inspector. One per cent. error in the readings of either current or volts, or ½ per cent. error in the readings of both of them, is sufficient to reject the machinery. How is it possible for an inspecting engineer to avoid dispute? how can he say that he

feels more confidence in his instruments than in those of the maker? The balance of probability is that the maker's instruments, having remained *in situ* since their calibration, are less liable to error than the travelling instruments, which may have received shocks during their journey. If, however, the dispute had not been an electrical one, but had turned on questions of size or weight, the size measurements would have been easily dealt with by the ordinary steel rules in the shop, which can generally be depended upon to one part in a thousand; and the weight would have presented no difficulty, as, if an ordinary beam balance were available, it could be adjusted in the presence of both parties, or, at any rate, the positions of the standard weight and of the object to be weighed could be exchanged, so that the mean of the weights thus taken would give the correct result. I have always thought that all important and accurate electrical measurements should be as easily checked and referred to a standard as the measurement of weight can be in the beam balance, but no existing measuring instruments having moving parts can be thus easily checked. For this reason, for the past eight years I have been attempting to reduce all electrical measurements to a balance of electrical forces—that is to say, by referring them to a standard fall of potential obtained on a carefully calibrated slide wire—in other words, the potentiometer. I have occupied much of my leisure time during the above period in perfecting this potentiometer system, which is already well known to most of you. I only mention it here in order to point out that with this instrument there are no moving parts, and that the only calibrated portion—the slide wire—can be subjected to extremely rough treatment without altering the accuracy of the instrument to any material extent. Once this slide wire is accurately divided, and that portion of it on which the sliding contact works is made of equal and even resistance per unit length, and is stretched rigidly over an accurately divided scale, it is almost impossible for the instrument to lose its calibration, short of actually severing the wire; and even if the wire is severed, a new wire can be applied and calibrated without the necessity of sending the instrument home to the maker, or to any standard laboratory, in order to compare it.

Whilst on the subject of electrical measuring instruments, I must not neglect to notice that very beautiful instrument, the bolometer of Professor S. P. Langley, and its recent developments in the hands of Messrs. Lummer & Kurlbaum. The bolometer is really the most refined method of measuring temperature that has yet been proposed. Its readings are obtained by measuring electrically the varying resistances of a platinum film when submitted to minute changes of temperature. It is, in fact, Siemens's electric pyrometer or Callendar's platinum thermometer in its most complete form.

I should also not fail to call your attention to the splendid work of Professor Callendar and Messrs. Griffiths & Clark in perfecting the platinum thermometer. By the aid of this instrument, and by electrical means, Messrs. Griffiths & Clark carried out the most recent determination of J, the value of the mechanical equivalent of heat. In my own practice I have found that the ease with which the platinum thermometer can be used in connection with my potentiometer system has given to me the most accurate, sensitive, and convenient methods of measuring temperatures in the various electrical apparatus which I have to examine. Anyone who possesses a potentiometer can take almost simultaneously any desired number of temperature readings by employing a number of platinum coils all made equal in resistance to an external standard with which they have been compared and adjusted at the temperature of melting ice. The external standard must, of course, be of manganin, which at ordinary temperatures has practically no temperature coefficient. If we adjust our potentiometer so that the readings on this standard are equal to 100 on the scale, the readings taken over the various platinum coils that are coupled in series with it will give directly the percentage rise, from which the temperature can be read off on a table calculated from the temperature coefficient of that batch of platinum wire.

I do not know if this beautiful method of measuring temperature is generally known to electrical engineers who are investigating such matters as the changes of resistance due to changes of temperature in insulating materials, the rise in

temperature of the various parts of dynamos, transformers under varying conditions of load—in fact, in the everyday quantitative work of the electro-dynamic designer.

I have stated that in my researches I have adhered rigidly to the principle of making accurate electrical measurements by obtaining a balance of E.M.F.'s—in other words, by using a zero method. I should here explain that there are two zero methods—namely, that obtained from the simultaneous balancing of two forces, as in the Wheatstone bridge or any other split-circuit device; the other, by balancing each of the two forces in turn (*i.e.*, the one to be measured, and a standard with which it is to be compared) against a third force, the two comparisons being made as quickly as possible one after the other. This is the zero method that I prefer, as it avoids the very great possibilities of error which I find are likely to arise whenever divided or shunt circuits are used. With this latter method the determination of several falls of potential can be obtained off different portions of the same circuit through which one and the same current flows in series. One can be certain that, apart from the minute variation due to the difference of time, the conditions of current-flow are absolutely identical. From my point of view the only permissible use of shunt circuits is in the potential leads attached to the two points, and by means of which the difference of potential is measured; but a circuit through these leads cannot strictly be called a shunt to the main circuit, for, although a small current may be passing through it before perfect balance is obtained on the potentiometer, as soon as such balance is obtained absolutely no current passes through this potential lead circuit, so that any change in their resistances, or in the resistance of their contact with the standard conductor, does not affect the readings. For accurate measurement of large currents we use sheets or strips of suitable alloys, so as to reduce the correction for change of resistance due to change of temperature as far as possible. The copper-manganese nickel alloy called "manganin" has turned out to be very useful to us in this respect. It has the great advantage of having a falling curve between the temperatures of 30 and 70 centigrade, and can consequently be compensated by

being combined with a metal having a rising curve, in such a way as to produce a standard having no temperature correction necessary between the above limits.

No doubt you will expect me to say a word on the subject of electric storage. Although comparatively little has been written or said about the improvement in the design and manufacture of the lead secondary battery invented by Planté, yet the progress in its development has been quite as marked and quite as satisfactory as that of the dynamo machine or alternator. I do not allude to new patterns of accumulators which claim to be new inventions, but solely to the improvement in detail and in manufacture which has followed on the competitive use of well-known existing forms. No doubt one reason why members of this Institution are not kept as well informed on the development of the storage battery as on other subjects is, that the questions involved, being electro-chemical ones, are extremely complex, and require long periods of time for their thorough investigation. Moreover, I regret to say that a dead set has been made at accumulators by some members of this Institution, who never rise in their places to mention them otherwise than by a sneer. It is curious this should be so. The secondary battery demands as high qualities for its patient investigation as does any other branch of electrical engineering; and those who are never tired of telling us that they cannot see the importance of improving electrical storage, as much as that of any other portions of electrical plant, only show that they have a very incomplete grasp of the requirements of our art. At any rate, if we can judge from cost price and from cost of upkeep only, the advance in the construction of accumulators during any period—say the last ten years or last five years—has been quite as rapid as that of the advance of the construction of dynamo-electric machinery. A reference to the price list of makers of dynamos and accumulators will show that this is the case, and from my own knowledge I can state that recent improvements are reducing the cost of maintenance to an equal extent. The study of the proper application of accumulators for electric supply is not confined to the design of accumulators themselves, but to the many adjuncts necessary to use them to

the best advantage. The difference between the E.M.F. of charge and discharge has been satisfactorily dealt with by the various booster systems that have been introduced during the last two or three years; and the proper proportion which the output of the accumulators themselves should bear to that of the station, and the most economical size of generating unit to charge these accumulators, have also been carefully worked out.

It is probable that most of the early troubles with accumulators were due to want of knowledge of the necessity for extreme purity in the materials used. These impurities concentrated themselves in the active material on the negative plate, and caused the now well-known phenomenon of self-discharge of the negative plates, which of itself is sufficient to account for the majority of the early breakdowns which at one time appeared to be so mysterious. We know how important it is for dynamo construction to have iron absolutely free from manganese and copper from arsenic, minute quantities of these which could hardly be detected by chemical analysis having the effect of largely altering the permeability of the iron or the conductivity of the copper. No less it is the case that minute quantities of certain metals, such as copper and arsenic, have an equally prejudicial effect in the lead used for accumulators; and the discovery of this effect has had a most important effect on the cost of manufacture and the cost of upkeep.

I feel that I should be failing in my duty towards you if I did not raise a note of warning on a point on which I feel very strongly. Owing to a combination of causes, the chief of which has been the great extent to which public money is now being spent on electrical supply undertakings, the control of large sums raised on the security of the public rates has been placed in the hands of very young engineers whose engineering life and experience has been a very short one. I am afraid that, in some cases, the extent of their responsibility has not been as fully realised by these gentlemen as it ought to have been. Local and special requirements have been insufficiently studied, designs have been hurriedly prepared, the *pros* and *cons* of the various systems which could be used have been inadequately balanced,

and I feel great misgivings as to the criticism that will be passed on these hurried designs after a few years have passed. I go further than this: I think that in many cases failure in part or in whole will follow; and so soon as this takes place a reaction will set in, which will damage the interests of our profession almost as much as the speculation in electric light companies' shares in 1882. We all know, or ought to know, that an electric supply system, to be successful, ought to be worked out from beginning to end as a complete scheme, with due regard to the special conditions of the town as to probable light and power requirements, and to the habits of the people; and that this is a very different thing from the mere preparation of a design for a building containing some plant, for laying a few miles of mains in the principal streets, with variations in the shape of providing power from the dust destructors or utilising thermal storage, in order to humour the vagaries of those of the electric lighting committees who fancy themselves engineers. I fear that this Institution is in some degree to blame for this: the papers that have been read, discussions that have taken place, have turned far too often on purely scientific matters, of interest only to the scientific men who took part in them; and that this has thrust into the background the more prosaic matters which must be studied by those who hope to deal thoroughly and economically with the many and varying conditions of electric supply.

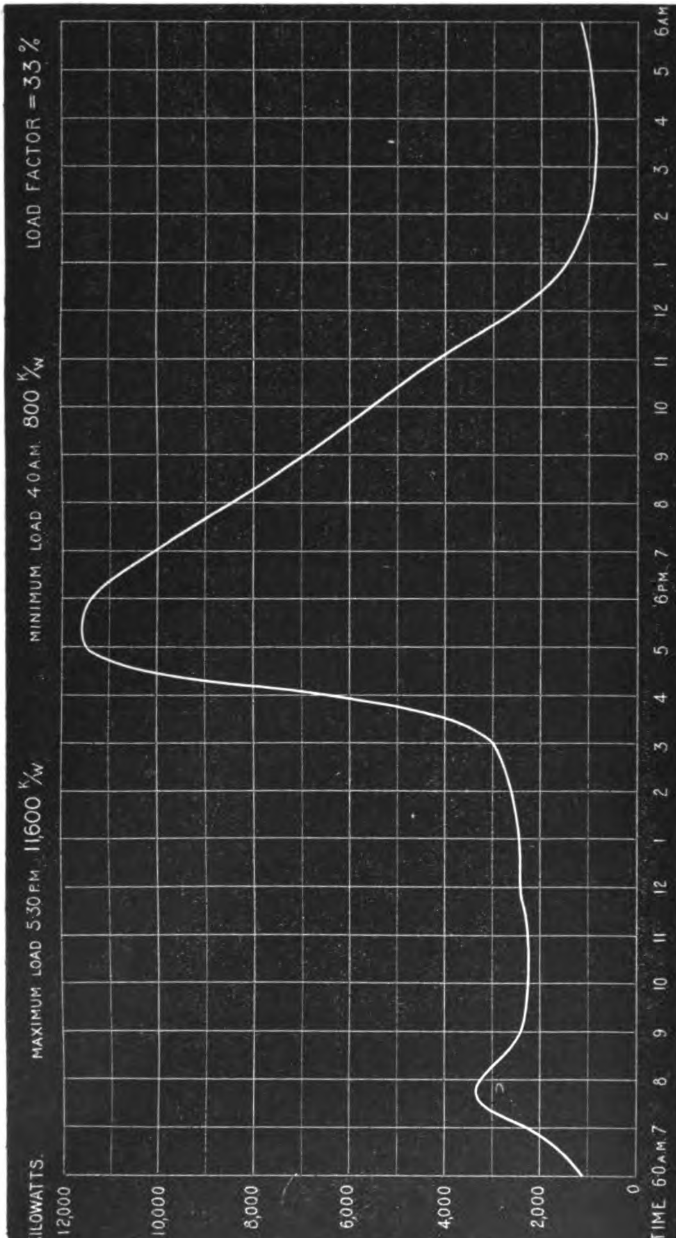
I hope these remarks will be taken in the sense that they are intended to be taken. It is even now not too late for those who are already carrying on works to inform themselves more fully than they have hitherto done on what is going on around them, and on the respective merits of existing electrical engineering practice. I know that many of those who are now so engaged, and who are responsible for the designs of the supply works that are now arising throughout the United Kingdom, and many of those who are now on the look-out for such employment, are setting out insufficiently equipped for their task; they have, I fear, studied their subject too much as book students, and have not spent sufficient time on travel and on the careful study of

electrical machinery as it exists in England, the Continent, and America. As an engineer intimately connected with the management of several important supply works, I know that the number of young engineers who really attempt to practically examine and study the working of existing supply works is small. If I ask the engineers in charge of these works how often they receive visits, or have really serious inquiries addressed to them on these matters, I am answered that such inquiries are practically not made; in fact, I am afraid that the interchange of ideas, the comparison of experiences in the use of various novel kinds of plant, the exchange of statistics, exists chiefly among us, the older members of the profession, and that, although we are most willing to impart what we know to our younger brethren, we are practically never asked to communicate the results of our experience.

I hope, also, that what I am now saying will not be put down to the natural conservatism or grumbling of an older man dissatisfied with the existing order of things. Think the matter over for yourselves; compare the length of time that you have occupied in your training, and the number of years' experience that you have in your new profession, with that of the engineers employed in other branches of engineering who are put in charge of undertakings of equal importance. However brilliant may be your talents, you cannot hope to acquire that unerring critical faculty, that power of rapid decision between two methods that offer themselves to you, of dealing with an engineering difficulty, and which was so ably described and put before Section G at the last meeting of the British Association by one of our members—the president of that section, Professor Kennedy—until you have added years of practical observation to your training as a pupil, or your workshop experiences. I feel sure that Professor Kennedy will allow me to quote his own words: "The whole use of college "training, of workshop practice, of practical experience, is to "provide the engineer later on with the means of critically "examining each question as it comes up, of reviewing systemati-"cally the *pros* and *cons* of each method of dealing with it, of "coming finally, rapidly, positively, to some definite condition, "which may then be irrevocably carried out."

I am now addressing the whole body of electrical engineers, old as well as young. None of us are too old to learn and profit from the experience of others; in fact, as I have already said, I find that such interchange of ideas as now exists is far more among the older than the younger members of our profession. This is much to be regretted. It may be partly due to the diffidence or shyness of the young men; but I greatly fear in many cases it is due to their fear that their crude and hastily formed ideas may be adversely criticised. Never during the 16 years which I have named as the life of our art have there existed more momentous questions to be solved than at the present moment; and if I can by my words induce a freer interchange of ideas, so that we help one another more than has been done in the past, I shall feel that I have been of some service to the Institution. Such interchange takes place most naturally in the tea room after each of our discussions. I think the time spent in the tea room is very valuable, and is far too short; and I think the practice, recently started by the Institution of Civil Engineers, of prolonging the evening meeting in the form of throwing open several rooms in which may be exhibited models or new instruments of interest, is a good one, and might be with advantage copied by us. This time after the meeting is often much curtailed by the length of the discussions, and I hope that you will support me in the chair when I try to shorten the discussion, particularly when it runs away on matters irrelevant to the paper of the evening.

The ideal method of best spending the time of the session is in discussions on the points of most interest to us at the time—*i.e.*, on burning questions which might be started by comparatively short papers, printed and distributed before the discussion, so that the whole evening might be devoted to it. I, for one, hope that we shall have free, fearless, and open discussion on all matters which affect our art. Let us keep away from any imputation of personal motive, but at the same time let us not hesitate to condemn as well as to praise, simply because such condemnation may appear to hurt the commercial interests of A, B, or C; I say advisedly "appear," because honest criticism in most cases



Load Curve, showing the combined Output of the whole of the London Central Stations during the 24 hours ending 6.0 a.m., on the 20th December, 1894.

is likely to prove to the advantage of A, B, or C, as it may open their minds to matters which they had never had fairly put before them.

After such a long spell of what I am afraid you will consider too plain speaking on my part, I end my Address by calling your attention to the diagram on the wall, which shows the combined output, or load, of the London electric supply stations for a night in December, 1894, and which was taken simultaneously for the purposes of this Address, and for which I have to thank the engineers of the supply companies. I have not had the opportunity to compare this with the load of any other city in the world, but I fully believe that it will be found to be one of the largest that has ever been drawn out; at any rate, we electrical engineers, with such a diagram before us, can point, at all events, to the very substantial degree of development of supply of electric energy in London.

I have to thank you for listening patiently to what, after all, is a recapitulation of facts which have been already known to many of you; I can only hope that they were not known to others, and that they may be of use to them.

General WEBBER: Sir,—It is my pleasing task to ask this meeting to render you a vote of thanks. I shall not address myself to you, as is usual in speaking to a meeting presided over as we are, but to the gentlemen present, in order that I may say a few words which it is possible your modesty might wish that I should leave unsaid. Gentlemen present, perhaps, have not come across the record of Mr. Crompton's career in the way that I have had the good fortune to do. I first had the pleasure to meet him at Paris in the year 1881, with a Crompton-Bergen dynamo between us. But there is a record before that date which deserves mention. I do not know whether Mr. Crompton—born in the premier county of England, where everything is best (at least, so Yorkshiremen tell us)—was one of those infant prodigies that our uncles and our aunts tell us are geniuses, destined to receive salaries of £500 a year, because they have always been "playing with electricity and "things." I daresay we have all come across them. But I find that in the year 1864 he joined the Army. He joined what we

are all proud to call "that thin red line,"—although I daresay he will correct me, because he joined a "thin green line," viz., the ranks of that celebrated corps, the Rifle Brigade. I will go a little back in history, and remind Mr. Crompton that the green line inherited its traditions from the old 95th Regiment, the 3rd corps of that splendid Light Brigade that made the celebrated forced march of 50 miles before the battle of Talavera. They were then red; afterwards they became green. As one colonel of his corps—the late Sir Alfred Horsford—once told me, when the red line was stretched across the valley of Inkerman, at the soldiers' battle, and his battalion of the Rifle Brigade was sent in amongst them, one company sent here by Sir George Cathcart, one there, and one there, the battalion in green jackets became merged in the "thin "red line," and fought side by side of them for the rest of that day.

Now, gentlemen, in his military career Mr. Crompton spent some nine years in India; and we find that even then, as he has told us, though in very few words, he had already turned his attention to mechanical engineering, with such success that his services were placed by the Government of India at the disposal of the Postmaster-General for the purpose of organising road traction, which was then intended to have been a substitution for bullock transport on roads for the conveyance of heavy mails, and even passengers. Mr. Crompton for four or five years was entirely in charge of that service, and when he left it he received the thanks of his Department; and it is very probable that the experience he then gained, and the service he performed, were conducive to his success afterwards; and he left a record, not only in his regiment, but in India, as being one of the most useful officers of his time. That "thin red line" I have mentioned (Mr. Crompton is the third President recruited from it) is a thing we are all a little proud of; and what does it mean? I have always looked upon it to mean this—that Englishmen will not fight one behind the other; they have always fought in line. They do not like column formations, and they like to fight shoulder to shoulder. Now, in Mr. Crompton's career as an engineer, I would claim for him that, while never wishing to be further in advance of others than his

merits would justify, he has always wished to be in the front line in his profession; and that he has been up to that line, not only has our long and intimate acquaintance with him as a friend and engineer proved, but it is also shown by the Address that he has been good enough to give us to-night. I hope Professor Forbes, who I see has been taking notes during this Address, will thank me for doing what I always wish to do to the gentleman who seconds me in a pleasing duty of this kind. I do not expect that I have trenched in any way on what he is going to say, because the object of my speech has been to bring before you something of the past life of our new President, and not to refer to the Address, which Professor Forbes is probably better qualified to deal with than I am. My privilege it is to ask you to say—and I know you will do it by acclamation—that the best thanks of the Institution are due to the President for the able and instructive Address just delivered by him, and that he be requested to permit its publication in the Journal of the Institution.

Professor FORBES: In seconding the motion that has been proposed by General Webber, I wish to say a few words on the subject of the very interesting Address which the President has given us. I think everyone here must have experienced the same feeling which I have whilst listening to it, viz., that it is one which is going to tell. It contains words of warning and words of advice, not only to electrical engineers—to us here present, and to our *confrères*—but also to the outside public; and I feel that his words are going beyond these rooms, and will be heard of afterwards. I think that all who have gone any length in the art of electrical engineering will feel a cordial agreement with what Mr. Crompton has said this evening on various subjects. I cannot recall at this moment a single case where Mr. Crompton and I have had any differences of opinion, but certainly this evening I am in the most hearty agreement with all the remarks that he has made on the various subjects touched upon. His first remarks were with reference to the development of the apparatus which we use. I take exactly the same view which he has done, and I think all those who have studied the history of

electrical engineering will do the same—that is to say, that the great development has come from the time when the dynamo began to be constructed in a thoroughly mechanical way. The invention of sheets of iron instead of wire for the armature, and the discovery of the necessity of driving mechanism for the armature wires, are two of the great mechanical points which had to be attained, and which are attained, by mechanical people. At the same time, I agree with him also that we owe an immense deal to those experimentalists and instrument makers who undertook the construction of the first dynamos; and it is owing to their having insisted upon the methods which attained the greatest efficiency, as they thought, that so much higher efficiency has been reached than in steam engines or other machines, and that we have not been content with the purely mechanical type of construction, such as in the old Brush armature with the solid iron core. Among different subjects which Mr. Crompton has spoken of, I will only mention one or two which seem to me of exceptional importance; and, first, let me call your attention to the remarks he made about the load-factor. Mr. Crompton has *par excellence* the right to speak on the subject of the load-factor. Many had noticed—of course everybody had—the trouble which came from the irregular shape of the daily curve; but it was from the date of Mr. Crompton's paper before the Institution of Civil Engineers that the full importance of the study of this curve, and of the means for ameliorating it, were fully appreciated by the electrical engineer. It has been interesting to hear from one who has been studying the best way of improving those load curves so thoroughly, the various means which he thinks are at our hand to develop the use of electricity during the hours when at present it is at a minimum; and his remarks on that score are not only applicable to the electrical engineer, but also to the public at large. What he said to us will be most impressive to the public as to the economy of using electricity for cooking and various other purposes. I agree with Mr. Crompton that certainly in a great many cases the economy of transmitting power through shops and abolishing a great portion of the shafting is higher

than in the particular cases which had been mentioned by Professor Kennedy. Certainly in a large number of shops losses through transmission by shafting must be even in excess of what Mr. Crompton has stated. It is needless to mention the various points that he has alluded to in connection with Mr. Ward Leonard's method, which is only one amongst others which have been adopted for varying speed torque ratio.

I thoroughly endorse the remarks Mr. Crompton has made—words of warning to the younger members of the profession not to deal too hastily with the problem that is before them, as to the arranging of any central station, or other work of that sort,—that no amount of pains spent upon preliminary study of the different methods by which the result can be attained, is wasted time; and I only trust Mr. Crompton's fears may not be realised, and that we shall not have cases of collapse for our profession to suffer from in the early future. In conclusion, I wish to emphasise his final remark—that is, that we all here do wish—I think I may say heartily—to support the Chair in the endeavour which our President intends to make to preserve discussions in an orderly and correct manner, and, while avoiding personalities, to allow the freest discussion—where it is really discussion—to the point before us. I am sure Mr. Crompton may rely upon everyone here giving him their most cordial support in every effort that he makes to maintain the dignity of the Chair in this Institution. I beg to second the motion of General Webber.

Mr. Alexander Siemens: It is hardly necessary for me to put this resolution to the meeting; but, for formality's sake, I will ask you to show your approval of the resolution proposed by General Webber and seconded by Professor Forbes.

The resolution was carried by acclamation.

The President: Gentlemen,—I stand before you a very proud man. I have now to announce that the scrutineers' report the following candidates to have been elected:—

Foreign Members:

Professor André Blondel.	H. Ward Leonard.

Associates:

William John Aubert.
Ernest E. Bartlett.
William Church.
E. S. Curtis.
Charles Manners Donnie.
Reginald Falshaw.
John Raven Frankish.
Julius Frith.
Alfred Sharman Giles.
Arthur Hattersley.
William Jones.
Lionel James Langridge.
A. B. Layton.
Samuel Arthur Mahood.
George W. Money.
Joseph Allison Newton.
Alfred William Sharpe Pocklington.
Carl W. Schaefer.
Alfred Schwartz.
Henry John Wagg.
Arthur Thomas Walmisley.

Students:

Dennis H. Bayley.
Frederick Simmons Grogan.
Luis Ebenor Hazera.
C. McCarthy-Jones.
H. J. Saunders.
John Ortelli Zerega.

The meeting then adjourned.

The Two Hundred and Seventy-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, January 24, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on January 10th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Brierley H. Collins.

From the class of Students to that of Associates—

Herbert S. Austin.	Joseph McMahon.
George A. Bruce.	George O. Sedgwick.
James A. B. Horsley.	Alfred Richmond Sillar.
Frederic Osmond Hunt.	Hubert Conrad Sparks.

Mr. Simpson and Mr. Fleetwood were appointed scrutineers of the ballot for new members.

Donations to the Library were announced as having been received since the last meeting from the South African Association of Engineers and Architects; Mr. Killingworth Hedges and Professor Andrew Jamieson, Members; and from Mr. G. G. Ward, Local Honorary Secretary for the United States, a framed portrait of the late Cromwell Fleetwood Varley; to all of whom, on the motion of the President, a vote of thanks was unanimously accorded.

The following paper was then read:—

THE ORIGIN AND DEVELOPMENT OF THE TELEPHONE SWITCH-BOARD.

By J. E. Kingsbury, Associate.

Mr. Kingsbury. The telephone itself has been the subject of various papers read before this Institution, but the same remark does not apply to apparatus which has become allied with the telephone in its commercial applications. The most important of such apparatus is the switch-board used in exchange service. It is because this subject does not appear among your records that I have treated it in the manner indicated by the title of this paper. To those who have been long engaged in telephone work such a title will not raise expectations of novel information, or suggest exhaustive treatment. To those who are not or have not been so engaged I can only hope that it may be of interest to the same extent that it is of interest to trace the development of any enterprise or apparatus.

The switch-board has been described as the brain of the telephone system. In seeking anatomical similes I hardly know whether to choose the brain, the heart, or the backbone. It is the brain in so far that it is the nerve centre, all communications going through it; it is the heart in that it is the life of the telephone system as we know it to-day; and it is certainly the backbone—of the exchange business.

The telephone, itself a marvel of simplicity, has become involved in a network of considerable complexity known as an "exchange system"—a means of inter-communication of which the switch-board is the centre-piece. The exchange system seems necessarily identified with the telephone, but it existed before the telephone was invented.

I am indebted to American sources for the information that the first idea of exchange working is to be found in a British patent granted to an inventor bearing a French name. Mr. T. D. Lockwood says: "The first idea of exchange work which, to my "knowledge, occurs in history, is found in a British patent,

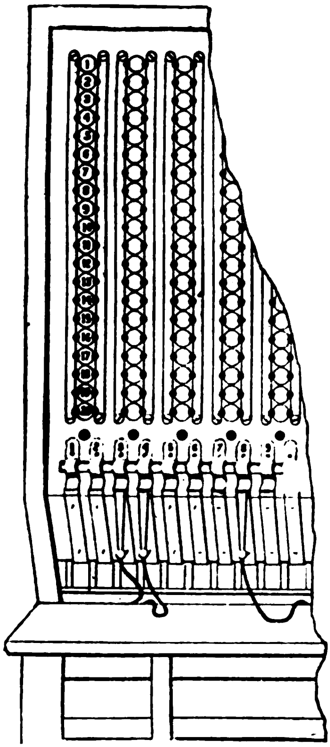
"issued as far back as February 7th, 1851, to F. M. A. Dumont. "This clearly describes an exchange system of dial telegraph "instruments, connecting by short lines with branch central "stations, which branch stations were provided with trunk lines "connecting them with a principal station." Mr. Lockwood remarks that "the idea was far ahead of the times."—(Am. Tel. Con. Rep., 1887, p. 50.) Mr. Kingsbury.

At Newcastle the Post Office arranged a system of connecting subscribers provided with Wheatstone A B C instruments. In New York such a system was introduced in 1874 or 1875. The founder of that system was not aware that inter-communicating exchanges had previously existed. He had observed the success attending the telegraphing of stock quotations to subscribers, who each received identical information despatched from a central office, just as is done by the Exchange Telegraph Company here, and he assumed that a like success might be obtained by furnishing information to legal firms as to the course of proceedings in the Courts. The legal firms did not take kindly to the idea, but one of them offered an alternative suggestion, intimating that a system which would enable lawyers to communicate with each other by telegraph would be worth paying for. Confirmation of this idea was obtained from others, and the result was the establishment of a telegraph exchange. As the exchange was exclusively used by legal firms, the company was called the "Law Telegraph Company." Telegraph instruments were subsequently replaced by telephones, and the particular system adopted for working the exchange became known as the "Law" system.

Notwithstanding that this telegraph exchange was already in existence at the time of the invention of the telephone in 1876, it was not until January, 1878, that the first commercial telephone exchange was started. In the meantime, Bell's invention had been greatly improved, and shortly afterwards the introduction on a commercial scale of the Edison carbon telephone and the Blake transmitter—an early and admirable modification of Hughes's beautiful discovery—placed telephony upon such a practical basis that its progress became assured. It was the microphonic era

Mr Kingsbury. when was commenced that activity in telephone exchange work which necessitated attention being given to switching mechanism.

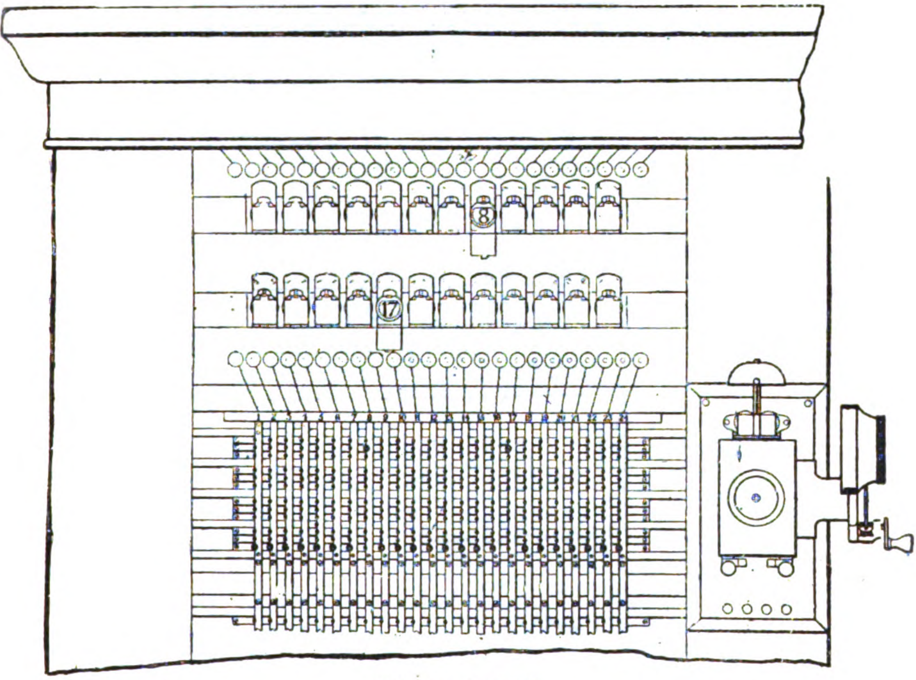
In addition to the public exchange system, every large telegraph office was the centre of numerous lines which were connected together as required by means of a switch-board. Part of a well-known telegraph switch-board is illustrated here. It consists of vertical and horizontal bars, the vertical bars being the lines, the horizontal bars providing the means of connecting any one vertical line with another by inserting plugs. The vertical lines terminate in springs resting on contacts. A telegraph instrument can be looped into circuit by a double-contact plug inserted between the spring and its contact. These springs were known to telegraphists as "spring-"jacks." Those who started telephone exchanges, like all other founders of new industries, adopted in the first instance that which already existed, so far as it was, within their knowledge, available for their purpose. All the first telephone switch-boards have been traced by competent hands to a telegraphic origin. Hence the origin of the telephone switch-board is the telegraph switch-board, with the addition of indicators already in use for signalling purposes.



Telegraph Switch-Board.

It will be remembered that telephone exchange business was started almost simultaneously in London by two different organisations—the Bell and the Edison Companies. The Edison Company had an experimental exchange at No. 6, Lombard Street. It was publicly opened on September 4th, 1879, with 10 subscribers connected. The switch-board then used formed the subject of the

first specification for a telephone switch-board in the English Patent Office (No. 3794, September 20th, 1879). The elevation drawing attached to that specification is here reproduced. The switch- Mr Kingsbury.

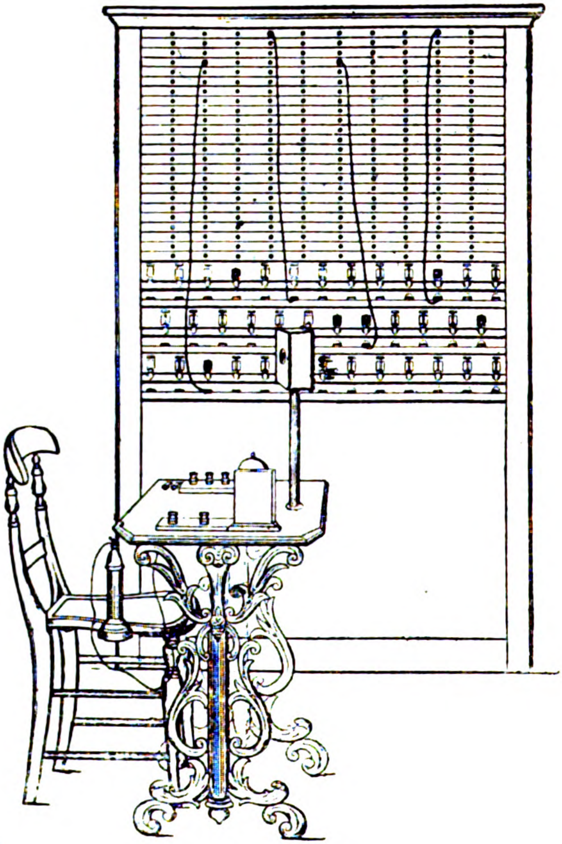


"Edison Board."

board used by the Bell Company in their first exchange at 36, Coleman Street, was of a different character. The sketch shown is made from a photograph in my possession. It is a rather curious circumstance that these two drawings, in addition to showing the first switch-boards used simultaneously in English telephone exchanges, are also examples of two types of board, viz., plug boards and cord boards, of which there were several varieties. The "Edison board"—as it is called here, because it was used by the Edison Company—is practically the same as the upper part of the telegraph switch-board. The vertical strips are continuations of subscribers' lines; the horizontal strips are normally insulated from the vertical strips; but any vertical strip may be placed in electrical contact with any horizontal strip by placing

Mr. Kingsbury

a metal plug in the hole which is drilled at the point of intersection. Another metal plug similarly applied will place the same horizontal line into electrical connection with any other vertical



"Bell Board."

line. Thus two subscribers' lines are connected together by means of a cross-bar and two metal plugs. There are no flexible cords whatever; even the speaking instrument is connected to a rigid bar. A different method is followed in the board used by the Bell Company, and consequently called here the "Bell board." The cross-bars remain, but the vertical bars are dispensed with. The subscribers' lines end in a spring-jack like the lower part of the telegraph switch-board, connections being made by means of flexible conduct-

Mr. Kingsbury.

ing cords having suitable plugs at each extremity. The one, therefore, is a plug board, the other a cord board; and for a while these two types provided the necessary material for the holding of opposite views amongst practical telephonists. The plug board was preferred because it was free from the liability of broken cords, had its construction and circuits readily traceable, and also, perhaps, because it was generally more in accordance with previous practice in switches. The cord board, however, had greater flexibility and possibilities of concentration.

Numerous varieties of plug board were made, differing considerably in appearance, but having exactly the same method of working. It is not possible for me, in the time at my disposal, to describe in detail the various differences. It will suffice to say that the general idea was the same; the differences were either in construction, or in the method of connecting the vertical with the horizontal bars. In some the plugs were loose, as in the one shown; in others—as in those originally used, I believe, in Manchester and Sheffield—the connecting plugs slid over a rod with notches for each horizontal bar, and were held in place by a spring clip fitting into the notch. These are merely differences of detail, and need not now be considered. It is of more importance to trace the methods of operation.

In the Edison board, whilst the majority of horizontal bars are free for use as connecting strips, two of them are appropriated for specific purposes. The seventh bar is connected to the operator's telephone, the twelfth bar to the earth. In order that each subscriber's line may be complete, so that the exchange may be called, the plug must be normally in contact with the earth bar. Where the fall of the indicator shutter shows that a subscriber is calling, the plug has to be taken out of the earth bar and set into the instrument bar. When the number of the subscriber required has been ascertained, the peg has to be again removed from the seventh, or instrument, bar and set into one of the other horizontal bars which may be unoccupied. The peg of the called subscriber is also removed from the earth bar and placed in contact with the horizontal bar already selected. The two subscribers' lines are then connected. No provision

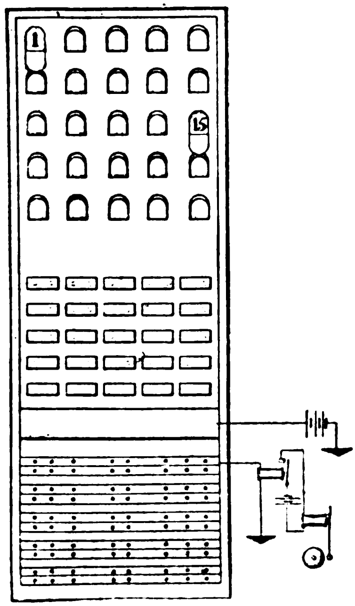
Mr. Kingsbury. being made for ringing from the exchange, one subscriber calls the other. This act throws down both indicators, which the operator must replace.

In the Bell board, the work is divided between two operators. One does the switching, the other the talking with the subscriber. The switching operator connects the subscriber with the table of the talking operator, who, upon learning the number required, instructs the switching operator to make the necessary connections, which he does with the flexible cords. Without detailing the various movements, it will suffice to draw attention to one point. The subscriber's line comes to a spring, and the spring rests on a contact. The contact is the earth. The plug with which connection is made is insulated on the under side, and removes the line from the earth; and that is one step in advance of the Edison board. A line needed to be broken and diverted. These requirements were separately met in the Edison board, whilst in the other case they were combined. In the development of telephone switch-boards that is equivalent to making two blades of grass grow where one grew before.

The cord board offered facilities in this direction which the peg board did not. According to the (American) Telephone Convention Report for 1883 (p. 33), plug boards were the most popular in 1881; there being amongst the companies reporting to the association 11,904 subscribers on plug boards, and 8,893 subscribers on cord boards. By 1882 the conditions were changed, cord boards having 16,799 subscribers, and plug boards 12,755. By 1883 the change was shown to have some substantial foundation, the figures being 21,432 for cord boards and 12,763 for plug boards. These figures do not refer to the identical boards illustrated here, but to all varieties of their respective types. They show that the intervening period was sufficiently active and exacting to have had some influence in deciding which was the fittest type of board for survival. That type was the cord board, and it may be desirable now to trace briefly the development of the most generally used form of cord board for small exchanges.

For this purpose it is necessary to call your attention to a patent specification which I think may be called a classic in this

branch of invention. It is dated November 29th, 1879, numbered 4903, and granted to C. E. Scribner. In Fig. 1 is represented a board known as the "universal "jack-knife switch." There are the indicators and the individual connecting springs for the lines as in the Bell board, but arranged in distinct groups, and there are the horizontal connecting bars also. Although there are these points of similarity, there are improvements over each. In the Bell board you will notice that the indicators and jacks are mixed up together, and the cords have to pass the indicators, but in the universal jack-knife switch the indicators are separated from the jacks. Both are arranged in distinct groups. In the Bell board the line comes to the indicator first, then through the spring-jack to earth. Inserting a plug into the spring-jack cuts off the earth, but the indicator remains in circuit. Scribner cuts off both drop and earth in one operation by the simple device of putting the drop the earth side of the jack, so that both subscribers' indicators are cut out of circuit instead of both remaining in. This is another step forward. The horizontal connecting strip is also improved by being turned round when engaged. This facilitated the selection of a disengaged connecting strip, but was not Scribner's invention. Mr. Kingsbury.



Scribner, 1879 (Fig. 1).

In connecting the two subscribers' lines together, two cords were used, as in the Bell board, each cord having a plug at either end, and the calling subscriber's line was connected to the revolving bar by one cord, and the same revolving bar connected to the called subscriber's line by the other cord. But, as both subscribers' indicators were cut out, some provision must be made

Mr. Kingsbury

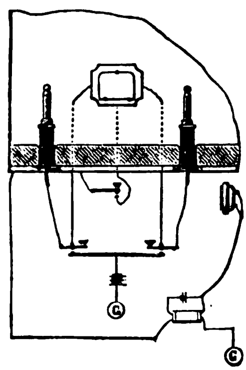
for the reception of a disconnection signal. This switch-board brings us one more step forward in the inclusion of signalling apparatus in the connecting cords or bars at the central office. And I would invite particular attention to the way this was done, because I think that it has been overlooked by some writers on telephone subjects. To each bar an indicator was connected in derivation, one end of the coil being connected to the bar, and the other to earth. The specification says: "The earth connection "of the coupling bar being through the coils of a magnet in "no way interferes with the telephonic conversation;" which remark you will apply to the lines then prevalent with the electro-magnet then used. The clearing-out indicator, as illustrated, was not mounted on the switch-board—there was a relay operating an audible signal; but a complete indicator is allowed for in the description. Although a clearing-out indicator was connected to the central office circuit, the operator's telephone was still at the extremity of a separate cord, and needed to be plugged into or out of a spring-jack in order to be brought into or out of the subscriber's line circuit.

The circuit made up by the operator for the purpose of joining two subscribers' lines consisted of two cords and a bar. There were the operations of putting two plugs into spring-jacks and two plugs into a bar. If the two cords were permanently attached to the bar, a part of the work would be saved. If, further, the operator's telephone were to be switched into or out of the line by a key, the operations required would be more quickly performed.

The rapidly increasing numbers in the exchanges rendered the bar system impracticable, whether of the fixed or revolving pattern. The bar therefore was discarded; the cords were lengthened, joined together, and the circuits so arranged that the ringing battery or generator, the operator's telephone, or the clearing-out drop, could be brought into use as required by the easy manipulation of keys placed in the most suitable position. This stage was reached in the switch-board now generally known as the standard switch-board. Its development dates from 1879, the germ being found in the universal jack-

Mr. Kingsbury.

knife switch. The keyboard was an entirely new feature in switch-boards, and was the logical result of the combination of connecting cords and plugs with clearing-out drops. The arrangement of the parts was strictly decimal—an obvious advantage hitherto neglected. The decimal arrangement is not shown in Fig. 1 of Scribner, 1879. The standard board was first made for 50 lines with indicators five across, as in the illustration (p. 43), but 10 deep, and the numbering was in columns downwards. That arrangement continued for some time, but was eventually changed to the numbering in lines as now adopted. Some of the keyboard circuits, being also applicable to multiple boards, were patented here, such as that shown in Fig. 2 of Specification No. 3116, 1883, whereby the operator's telephone is in circuit immediately a plug is inserted in a jack. This is done by means of the other plug of the pair resting on a plate to which the telephone is connected, and accounts for the metallic heel-piece to the plug which then came into use. This board has been greatly altered in the parts of which it is composed, but in general arrangement and in the methods of operation very little, if any, improvement has been found possible, and for small exchanges it has been extensively adopted, or imitated substantially in its original form. Sufficient thought had been bestowed upon it to enable it to assume a form based on sound principles. The board was upright, with a shelf for keys and plugs. What was needed to be seen or handled was placed in the front of the board, but nothing else. All the connecting wires were placed at the back of the board, so that they could be attended to by the mechanic without interfering with the operator. The importance of this arrangement is very apparent now.



Scribner, 1883 (Fig. 2).

The introduction of the standard board probably accounts for the figures which I have already referred to as indicating the change in general estimation of plug and cord boards. Although

Mr. Kingsbury.

the flexibility and concentration available with cords must have led to their general adoption, it is of interest to note how the placing of the operator's telephone into the cord circuit overcame one of the principal objections to cords. The existence of a broken cord must become known to the operator in the process of making a connection, and it could therefore be laid aside for another.

Whilst the standard board remains the most effective of what may be called "simple" switch-boards—that is to say, those in which a subscriber has only one spring-jack or point of access to his line—it also forms in its constructive and keyboard arrangements the foundation for the more elaborate multiple board, which, although previously invented, soon borrowed the general disposition and the simple and practical mode of operation of the standard board.

The multiple board as originally designed employed the Scribner "jack-knife" switch, which was never used in England, I think (p. 50). It consists of a metallic frame, with a blade pivoted at one end, and resting on an insulated stud at the other. There are two holes. These are for plugs—one for the instrument plug, and the other for the connecting plug. The insertion of either plug will lift the blade from the stud, and so break the earth and indicator contact. The importance of space on the front of a switch-board led to a change in the jack-knife switch at an early date. Instead of lying lengthwise across the front of the board, the blade or spring followed more after the fashion of the spring in the Bell board—*i.e.*, having its length from the rear to the front instead of from side to side. Though entirely different in appearance and construction, the modified switch was the equivalent of the jack-knife electrically. Whilst previous springs, as in the Bell board, were operated by flat plugs, the designer of the new spring-jack retained the round plug as used by Scribner, altering the shape so that it should be retained in position by the grip of the spring. This spring-jack and plug remain in their essential features the patterns now in use. The adoption of a round instead of a flat plug may seem a small matter, but it would require a very considerable amount of calculation to estimate the aggregate saving in time and money which has resulted therefrom.

Mr. Kingsbury.

It may be of interest to state that the inventor of the spring-jack and plug is Mr. J. C. Warner, one of the oldest of the expert staff of the Western Electric Co. Mr. Warner designed not only the jack, but also the indicator used on these switch-boards. He brought to the design of telephone apparatus a wide experience, and you will probably agree with me that he graduated in a good school. Born in London in 1822, Mr. Warner became engaged in the manufacture of electrical apparatus in the workshop in which instruments were made for Cooke and Wheatstone. Subsequently he made some apparatus for experimental use by Morse on the line between Baltimore and Washington.

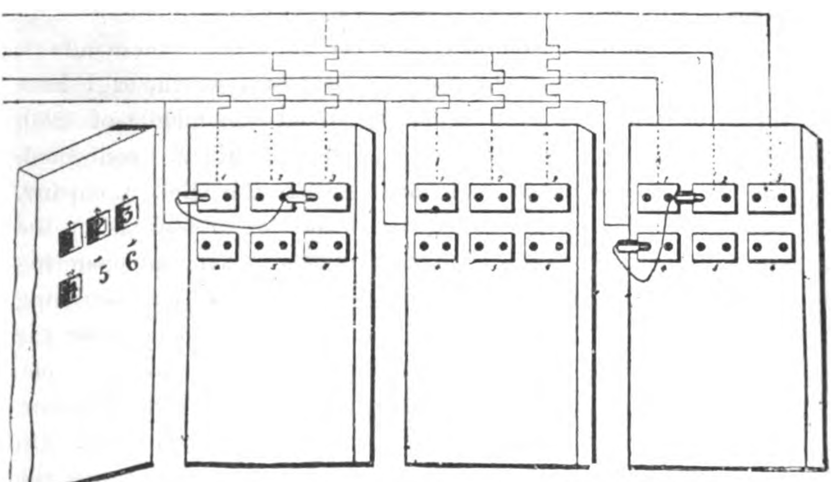
Having traced the simple switch-board to the stage in which it is supplied with an indicator and a spring-jack for each line, with pairs of plugs and connecting cords in whose circuit are placed convenient keys whereby the calling generator and the operator's speaking instruments can be quickly brought into circuit with either subscriber's line, consideration may be given to the more elaborate outfits required for large exchanges. It was on such a foundation that the multiple board was practically introduced, though it was designed at a much earlier period, and was being gradually matured.

For the purpose of illustrating the prior condition of affairs, I will recall attention to the Edison board, which, as I have mentioned, was installed in 1879. About the middle of 1880, when it had probably about 300 subscribers connected, the Edison Company amalgamated with the Bell Company. Some recommendations were then made for alterations in the switching arrangements. It was suggested that the operating should be divided between talking operators and switching operators, as in the Bell board, with the difference that the subscribers' lines should be continued to the talking table. Seventy-five lines were to be assigned to one talking operator, who was to be assisted by two others as switching operators. On the fall of an indicator the talking operator would depress the key pertaining to that line, would ascertain the number required, and then instruct the switching operator to make the required connections. If it should happen to be with a subscriber in

Mr. Kingsbury.

another section—say 200 in the third section—the switching operator would fix the peg in his own board, and then go to No. 3 board, fixing there the proper peg to connect No. 200 of that board with No. 1 of his own, using therefor the special connecting strips set apart for connecting each and every board individually with the other. It was asserted that "by this means "an exchange can never become unwieldy." How soon that anticipation became falsified, only those concerned with the practical work of exchanges are able to appreciate. The method of connecting separate boards by special "strips," or bars, was very much improved by "transfer" systems, as they were called when applied to the standard boards; but by dire experience it became evident that the rapidly growing exchanges could not be satisfactorily or economically conducted when there was only one connecting point to a subscriber's line, and when communicating conductors had to be made up by sundry operators to enable a connection to be made.

I will show you an attempt to provide a remedy. It is recorded in a United States patent (No. 252576), which was filed January 7th, 1881; and from the wording of the specification I gather that



U.S. 252,576, filed January 7th, 1881.

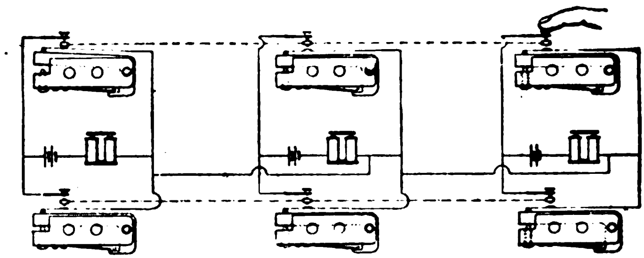
the system was in actual use. The subscribers' lines passed through a number of boards (it was on the "Law" system, and easily done).

The subscribers' numbers were inscribed on a signal board placed in a central position. These numbers could be obscured by shutters. When a subscriber was put through, the operator would instruct the attendant at the signal board to obstruct the number. On disconnecting, the reverse instructions would be given. Before connecting a line the operator would look at the signal board to see if the subscriber required were disengaged. This method was crude, but none the less deserves commendation as an attempt to overcome the difficulties which were becoming apparent. It is a multiple board without a test system. I cannot claim it as a link in the chain, because, as a matter of fact, the solution of the problem had already been in the Patent Office for over a year. It is in the specification to which I have already called attention—Scribner's, November, 1879. Numerous connecting points, without some prompt and certain means of indicating whether the line were engaged or disengaged, would be a remedy worse than the disease. A triangular conversation would be likely to produce more bad blood than a triangular duel. Now, in November, 1879, the exchange system in London had just commenced. It had certainly had a longer start, and assumed somewhat larger proportions, in the United States, but even there the necessity was not so great as to give rise to the idea which this invention conveys. Mr. Kingsbury.

The idea is really twofold—firstly, to enable one operator, whilst answering the calls of a given number of subscribers, to connect them with any other subscriber in the exchange without requiring the aid of any other operator or making up any combination circuit; secondly, to inform the operator of the condition of the line with which connection was desired – whether engaged or disengaged. The first part of the idea was attained by increasing the number of spring-jacks indefinitely according to the number of operators required to carry on the work of the exchange. The second part of the idea is really the establishment of an information bureau in connection with every line, the bureau being supplied with information automatically, and giving up its information on demand. Three ways are shown for giving this information: one is a visual signal electro-pneumatically

Mr. Kingsbury.

operated; another is the provision of double connecting blocks; the third is that shown in Fig. 5, which forms the foundation

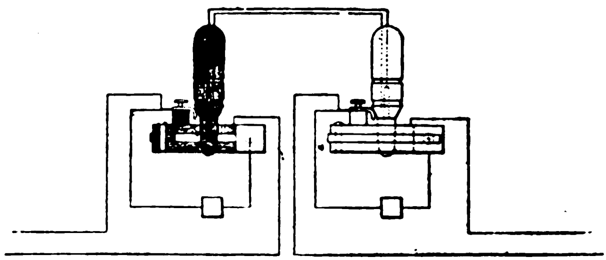


Scribner, 1879 (Fig. 5).

of the multiple-test system as practically developed. In the diagram only the local, or test, circuit is shown. The jack-knife switches illustrate two subscribers on three boards. When a plug is inserted in a jack (as is supposed to be the case in the first one illustrated), a spring, insulated from the blade, and connected with one pole of a battery, is brought into contact with a stud, which is connected by a line to similar studs on every other jack pertaining to the same subscriber. Over the studs are finger keys connected with the other pole of the battery. Depressing the key will close the local circuit and operate a bell or signalling instrument if the line is engaged, otherwise not. The information required was "engaged" or "disengaged." A local circuit partially closed at one jack would be completely closed by depressing the finger key at another jack of the same line: then the line was engaged. Depressing the finger key at one jack would not close the local circuit unless some other jack of the series had a plug inserted in it: then the line was not engaged. Here was no talking between operators, no passing of calls, or making up of lines. The information was obtained as the result of testing a local circuit, and hence the term "test circuit" as applied to multiple switch-boards. I believe that the invention in the exact form shown in the 1879 specification, as above, was only used in one exchange; but it was the subject of continued experiment and study, and was being systematically developed so that it might be ready when required. At this stage it had a test circuit entirely distinct

from the line, and a test key for each jack. We might reasonably expect that the development would be likely to take the form of simplification, and such was the case. Mr. Kingsbury.

Before I relate briefly the line of that simplification, I would like to remind you that we are dealing with a comparatively early period of the telephone industry—not too early to appreciate the advantages of metallic circuit lines, as contemporary records show, but a time when the commercial side of the telephone exchange business practically settled the nature of the lines to be run, so that single lines were the rule. It is possible that some may be surprised to hear that metallic circuit multiple boards are provided for in Scribner's 1879 patent, the necessary modifications to the apparatus being clearly described (Fig. 11) so as to adapt them



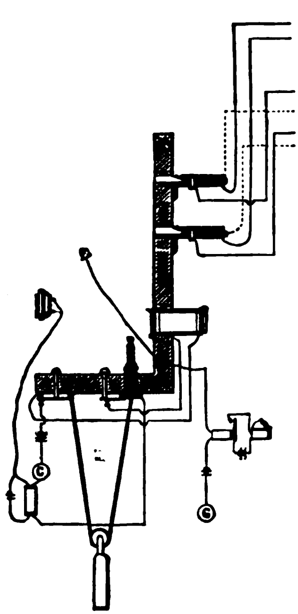
Scribner, 1879 (Fig. 11).

to metallic circuit use; the plan would not be considered quite satisfactory now, as only one side of the indicator is cut out. At any rate, switch-boards were required to be developed for single lines, and this enabled a simplification to be proceeded with. The separate test line gave way to a much more simple contrivance, which was communicated by C. E. Scribner, and patented here on 22nd June, 1883 (No. 3116).

To illustrate this improvement I must again refer to the spring-jack already described in connection with standard switch-boards. It consists of three parts—a frame, a spring, and a contact stud. As a standard board jack the spring is in metallic contact with the frame. To make this jack suitable for use on a multiple board it is only necessary to insulate the spring from the frame. Place a number of these jacks in a row, connect a subscriber's line with the spring of the first jack—the spring rests

Mr Kingsbury.

on the contact beneath—connect the contact with the spring on the next jack, and you thus have a line through the board which may be broken at any jack. This is the subscriber's line wire, which continues from section to section through the hammer and anvil contacts of the jacks, which contacts are mechanically sustained by the frame, though they are electrically distinct from it. If all the frames of a series of spring-jacks are connected together by another wire, you have two lines through the board—the subscriber's line and the test line. Insert a plug in any spring-jack and you break the main line, but connect together electrically the spring and the frame. In other words, the test line becomes a branch of the main line. The main line has an earth on it. Touch the frame of a jack (which is electrically a part of the test line) with a plug forming the extremity of a partial circuit including a signalling instrument and a battery earthed at one pole, and it will be seen that you complete the test circuit if the line is engaged, and you don't complete it if the line is disengaged. The same result is attained as by the 1879 system, but by a very much simpler arrangement of parts.



Scribner, 1883 (part of Fig. 1).

Another simplification was made in the apparatus for testing. The finger key contacts on each jack (1879) were so many branches from one side of a battery (p. 50). Instead of having numerous fixed branches, the same effect could be attained by having one movable branch. This was attained (1883), one separate test plug being provided for each operator. The test plug illustrated above is in the form of a thimble easy of application to the frame of a jack. The drawings show a bell, or buzzer, as a signal, but a "telephone" is suggested as an alternative; and it needed only a slight change to make a still further improvement, by discarding

Mr. Kingsbury.

the separate testing plug, making the operator's telephone the signalling instrument, and the connecting plug the testing medium. On touching the frame of a jack with the connecting plug the operator hears a click in her telephone if the line is already engaged. This method of testing by the operator arrived at the last-mentioned stage before the multiple board was put into practical operation in England, and so it remains to-day, although the electrical circuits are very different.

The early multiple boards were very much like overgrown standard boards, the cords being below the key-board. As then designed the cords were not long enough, and so in the Liverpool boards (the first used in England) the cords were overhead and longer. A board was supplied to the Globe Company in London at an earlier date (July, 1883), but the subscribers were not numerous, and I think that the jacks were not multipled; so that we generally regard the Liverpool board as the first in actual commercial use here.

After the Liverpool board, an important change was made in the introduction of what is variously called the "local section" or the "answering jacks." The latter term seems preferable, since it corresponds so well to the answering plugs used with them. The Liverpool board was arranged for 200 subscribers to a section, the spring-jacks being of the Warner pattern, mounted on wood. The jacks being arranged in blocks of 100, placed in numerical order, it will be seen that, taking, for example, the first section, the 200 subscribers' jacks of that section were all at the operator's left-hand end of the board, and all three operators had to reach those two jack panels. This difficulty was met by adding yet another jack for each subscriber—the answering jack. These answering jacks were spread out along the board, so that the answering was more promptly done, and an operator had not to reach past another in responding to a call. The arrangement of indicators in relation to jacks was rendered uniform in all sections —a great advantage in changing operators from one section to another. This plan was first introduced in the multiple board shown at the Philadelphia Electrical Exhibition. There was a further modification in the design which has sometimes been

Mr. Kingsbury. called a "double-deck" arrangement. At the top of the board were the general jacks, then the clearing-out drops, then a shelf containing half the plugs—one of each pair—then the indicators, below them the answering jacks, then another shelf with the remaining plugs and the keys. This double key-board arrangement has been retained, but the disadvantage attending the separation of the plugs became speedily apparent. Even so slight a change in the relationship of the plugs slowed down the operating, and their separation also involved a risk of using the wrong plug for completing a connection. The separation of the plugs was therefore abandoned. The plugs were both placed on the upper shelf, the clearing-out drops and the answering jacks changed places, and this arrangement of parts is still retained, except in the branching system, where the indicators are at the top of the board, and only one key-board is required.

Another problem was met in this 1884 board. Exchanges were growing larger and larger. More subscribers had to be provided for. More spring-jacks had to be placed on a section, the face of which could not be enlarged. The limitation of size in a multiple section is easily understood. It is the limit of reach of the outstretched arms. That is about 6 feet. The height is a little more variable, as the reach in that direction does not interfere with other operators. If, therefore, it becomes necessary to put an increased number of units in a given area, it can only be done by reducing the size of the units. A reduction in size of parts was then made, and further reductions have since been made. In fact, so far as designs and patterns are concerned, the size has been reduced so as to attain a capacity beyond the requirements of any exchange under existing conditions.

With the reduction in size came a radical change in the method of construction. Instead of each jack being made separately and then mounted on a base, the base and the jacks were incorporated. Twenty jacks were made into a strip, which was a combination of metal springs and insulating material. This was simply a constructive change, carefully carried out so as to make no change in the method of operation; the same round plug being used, and the same bush, carefully shaped so as to make the

Mr. Kingsbury.

act of testing easy and the completion of the connection quick. Although various changes in size and construction of parts were introduced after 1884, the general design and the circuits of the board were not materially altered for some years.

During these years the telephone exchange business had been very rapidly extending. Long-distance lines were being erected; overhead wires in some places were having such violent hands laid upon them that burial became necessary; the development of electric lighting and traction, and the use of powerful currents disposed to extend unduly their sphere of influence,—all these causes had their effect upon telephone switch-boards. Metallic circuit service had to be provided for, and, as the change must necessarily be gradual, mixed service had to be given first. This requirement was met with a minimum of change in existing apparatus. The earth circuit multiple had in the test wire a line which could be made available as a metallic return by a slight change. The engaged test, as you will recollect, depended upon there being an earth connected to the test circuit. Put an earth on a metallic circuit line when engaged, leave if off when not engaged, and you have the necessary dual condition for negative or positive information. This earth was attached to one or both limbs of the cord circuit, and formed a part of the local circuit, which was completed by the plug in the act of testing. Between the earth and the cord is placed a retardation coil, which prevents currents of high frequency from either entering or leaving the telephone line, but is no bar to the simple battery current required for the test. This may be termed a transition board, but it is a very practicable one.

The importance of accurately balancing the talking lines, as well as the certainty of the test signal, showed the advisability of going back to the original 1879 plan of using a test circuit entirely distinct from the line circuit; and I think that this importance was insisted upon by none more strongly than English telephonists. But here a difficulty presented itself. Experience showed that as the jack contacts in a board increased, so did the troubles due to poor contacts between the line spring and its contact stud. Supposing a 4,000-line board of normal dimensions

Mr. Kingsbury

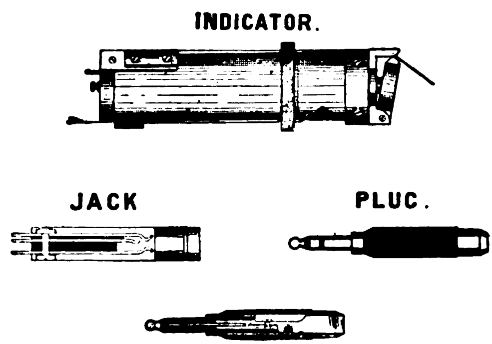
on the single or mixed circuit system, there would be 21 jack contacts in the line. To adopt the system of cutting out the indicator with metallic returns, and to employ a separate test circuit as well, would require three contacts in each jack; so that a 2,000-line board of such a character would have 33 jack contacts for each line, or about 50 per cent. more than a single or mixed circuit board of twice its number of subscribers.

With the general improvement in the quality of external lines, it is obvious that it would not do to put into the switch-boards apparatus of such a design as would lead to defects in the very portion of the circuit under the electricians' immediate control. This consideration led to another new departure—the last to which I shall call attention—and that is the arrangement of the subscribers' lines in an unbroken circuit throughout the board with no contacts whatever in the jacks, and the automatic restoration of the indicator. The branching of the line contacts and the use of bridging indicators in themselves are no new departure in switch-boards. The Post Office use branched line contacts and bridging indicators. It is of interest also to note that their indicators need no manual restoration. But the means by which these results are accomplished are not available for the methods of exchange working generally adopted. New means had to be invented. The idea had been "in the air" for a considerable time, but, as usual, lacked the final touches to make it practicable, like the invention of the telephone itself. I will indicate one of the principal difficulties. Let us suppose that two subscribers are connected, say No. 1 and No. 3000, the connection being made at No. 1 board. The indicator of No. 3000 is at the other end of the room. On sending a clearing-out signal, both indicators would drop unless cut out, as in the older system, or prevented in some new way. One of the earliest attempts at meeting this requirement was in Scribner's English Patent No. 20099, December 9th, 1890, which was one of the preliminary steps in the branching system so far as the jack contacts were concerned. The desirability of the separate line for test purposes, however, helped the solution. This test circuit might very well bear an additional burden. The

Mr. Kingsbury.

application of a locking device on each indicator, and controlled by a battery in the test circuit, was the first stage. The locking was a necessity; but it is only natural that it should have led to the restoration of the shutter to its normal position, thus rendering another of the switch-board operations automatic. The first design for the circuit of this board was by Mr. Bell, one of the switch-board experts of the Western Electric Company. But, in addition to the Western Electric staff, the design of the board was so fully discussed by the switch-board committee appointed by various telephone companies, that I am unable to give, as I should like to do, the individual credit for details.

The indicator, spring-jack, and plug are illustrated in the diagram. The indicator consists of two coils mounted on opposite



Branching System.

sides of a plate. The longer coil is the operating coil, and it is connected in bridge across the line. The short coil is the restoring part, and is connected in the test circuit. The operating coil when energised attracts the armature to which is attached the arm holding up the shutter. The shutter opens out if the test circuit is incomplete. But if the test circuit is completed by having a plug in any jack of its line, the restoring coil is energised and the shutter prevented from opening. The spring-jack has three springs, a bush, and a testing ring. The short spring is the contact for one limb of the line, the bush the contact for the other limb. The two springs of even length are the contacts for the test circuit. The plug has three contact surfaces.

Mr. Kingsbury. The point engages with the short spring, the sleeve makes contact with the bush of the jack, and the ring between the sleeve and the tip connects together the two test springs, thus closing the test circuit and restoring the indicator. The advantage of this construction is that there are no make-and-break contacts in the jacks whatever; and in this switch-board we reach the highest development so far in combining the most perfect talking circuit, a mechanism free from the liability of previously existing troubles, and at the same time a reduction in the manual work.

I will not weary you with a detailed account of the evolution of a spring-jack, but will be content to direct your attention to one feature—the testing point. In the first plan (Scribner, 1879, Fig. 5), the testing point was a small stud with a key contact. In the Liverpool board, the testing point was the front edge of the body of the jack, which also formed the channel for the plug (Scribner, 1883 diagram). In the Philadelphia Exhibition board (1884), it was still the front edge and the channel for the plug, but no longer the framework. In the branching system, it is neither the body of the jack nor the channel for the plug, but an additional metallic ring connected with the local circuit by a wire not shown in the diagram; but it still retains the front position because of the prime importance of accessibility.

Comparing 1880 with 1894, it will be observed that in an exchange to which I have already referred, having about 300 subscribers, it was considered necessary to provide a minimum of one operator to 25 subscribers. In the latest London switch-board to which I have referred, fitted for over 3,000 subscribers, there is a maximum provision of one operator for 50 subscribers; and in other places, on boards of the same system, 100 subscribers are attended to by one operator. The number of operators to subscribers is entirely dependent on the load line of the exchange, and there will be necessarily great differences, according to the nature and number of the calls. It will therefore be seen that the development in switch-boards has followed the general law of enabling more work to be done with less labour.

The latest switch-boards fitted in London are on the branch-

Mr. Kingsbury.

ing system, and it is needless to say that they contain many features originated by those who have the responsibility of their fitting up and maintenance. I hope the omission of detailed information on these points and many others will not be construed as an absence of recognition of the value of the work done outside the limited number of persons to whom I have referred.

I have advisedly limited this outline sketch to a record of what has been accomplished in the line of switch-boards whose merits are to some extent guaranteed by general recognition; and I have also advisedly omitted any reference to complex problems, such as trunking systems between separate exchanges. I do not propose to anticipate the result of experiments now in progress, or presume to speculate on the possibilities of the future, but I would like to be permitted to make one comparison of the present with the past. The progress of telephony has been the subject of some comment. There are differences in that progress, of course. Local circumstances and conditions will mostly account for them, but let us make a comparison without going away from home. Between the setting up of the first and the last telephone switch-boards to which I have drawn attention, there has been an interval of 15 years. We are accustomed to regard 1837 as the date of the introduction of the electric telegraph, although, of course, active work in a commercial way had not then, or for some time afterwards, commenced. Fifteen years after 1837 there was no public telegraphic communication between London and Brighton. The charge for a message of about 100 words to Liverpool was £5. These charges were regarded as high. "But no doubt," says a contemporary record, "these will be "diminished when the company's arrangements get more "matured, and they are better able to transact any amount "of business which may be offered to them. The directors "appear to be acting on the policy of checking, rather than "encouraging, the use of the telegraph by the public until their "establishments are everywhere in perfect working order." Now the most maligned telephone companies have never, I think, been accused of turning away customers. Notwithstanding the

Mr. Kingsbury. novelty of their business, they have been able to invite additions instead of requesting withdrawals. That is undoubtedly largely due to the extent to which the telegraph had prepared the way. Men experienced in the laying of lines and the management of instruments were already obtainable; but there is one direction in which there had been no previous experience of any moment, and that is in the methods and mechanism of inter-communication. The invention of the telephone was defined by judicial authorities as the creation of a new art. The greatest utility of that new art has been found in the development of that means of inter-communication, the exchange system, and the most important feature in the exchange is the switch-board. It has been a very easy task for me to pick out landmarks and put them together to show the route which was probably traversed; but members of this Institution are too familiar with such matters to need reminding that it was by no means so easy a task to devise and manufacture, or set up and maintain in working order, intricate apparatus which should keep ahead of the ever-growing public demand for telephonic communication. I think, however, there can be no doubt that but for the invention and adoption of the multiple switch-board there would be some parallel between telephonic communication now and telegraphic communication at the date I have mentioned: its progress would have been checked by the impossibility of doing the work required.

I have limited the illustrations in this paper to those which have not been published previously. The following references are appended for the convenient comparison of types mentioned but not illustrated.

An early form of the standard board is illustrated in Fig. 216, p. 314, of "The Telephone" (Preece and Maier), but it is there erroneously described as the "Williams board." A section of the Williams plug board is shown in Fig. 217, p. 315, of the same work. A later form of the standard board is shown in the "Manual of Telephony" (Preece and Stubbs), Fig. 215, p. 314.

An illustration of the first Liverpool multiple board appears in the *Electrical Review*, vol. xv., p. 312.

The Philadelphia Exhibition board is the same as that

illustrated in Fig. 229, p. 326, of "The Telephone," and the frontispiece of the same work shows the subsequent modifications in arrangement mentioned in the paper. Mr. Kingsbury.

The mixed circuit board and the branching system are both described and illustrated in the "Manual of Telephony" (Preece and Stubbs). Diagrams of circuits of the standard and multiple boards, as well as other illustrations and information of interest on the subject of the paper, will also be found in the "Manual of "Telephony."

The PRESIDENT: In rising to propose a vote of thanks to Mr. Kingsbury for his instructive and well-arranged paper on a subject on which I regret to say the majority of us know far too little, I have to remind you that he has an additional claim on our thanks, as he comes forward with this paper at a time when we were greatly in need of communications on this subject. The small extent to which telephone matters occupy the time of this Institution has been always a reproach to us. Even now the first important paper is given us by Mr. Kingsbury, who, I understand, is practically the representative of a foreign manufacturing company; but this does not lessen the debt of gratitude that we owe to him. I consider his paper is one of those which will take high rank as a reference paper, and will thus increase the value of our Journal. I therefore propose a hearty vote of thanks to Mr. Kingsbury. The President.

The motion was carried unanimously.

Mr. LANGDON, being called on by the President, said: I am sorry to have to admit that I am very little acquainted with telephonic switch arrangements; we use a very simple form of switch in railways. At the same time I am glad of the opportunity of saying how much I am indebted to Mr. Kingsbury for his paper, which is one of very great interest. Mr. Langdon.

Mr. C. J. PHILLIPS: I should like to make one or two remarks. First of all, I must say that I do not agree with you, Sir, that anyone other than Mr. Kingsbury should have brought this subject before the Institution. Those of us who are engaged in telephonic work, in London at least, will all agree, I am sure, that there is no man so competent in this Society, nor, I think Mr. Phillips.

Mr. Phillips. I might say, in the United Kingdom, as Mr. Kingsbury, to illustrate the particular subject he has chosen of the origin and development of the telephone switch-board, because there is no man in the country who has had equal opportunities of becoming conversant with it. There was just one little point I would draw attention to in the description of the Edison board. Mr. Kingsbury said that the subscribers were left to ring each other. That was, I believe, the original design of the board, but very soon after it came into use that particular method of working was greatly improved upon. Mr. Kingsbury mentioned No. 7 strip as being connected with the instrument. On looking at the sketch you will see that if a third plug—the two solid black dots near the top of the switch-board represent plugs connecting two subscribers together—were inserted between either of those vertical strips and the horizontal strip No. 7, the operator's telephone would be placed in connection with both those subscribers' lines, which gives her the power of ringing up either or both of them. And in practice subscribers' calls were dealt with in this way.

I think there is just a slight fallacy in Mr. Kingsbury's simile of "two blades of grass growing where only one grew before." I do not quite follow him in that. In the Bell board you had two operators at work—one to do the plugging, and one to do the talking. In the Edison board one operator did both—so far as the subscribers she was attending to and those on adjacent boards were concerned—and that was one reason, I think, for the preference shown at the beginning for the Edison board over the Bell board. It is a great feature in a switch-board to give the operator the sole control of the connections she has to make. If she is dependent on someone else doing part of the work, she does not know whether that other person has done what she desires; but if the whole of the operations are in her own hands, she works with more confidence, and fewer mistakes are likely to occur. The actual work to be done, I think, was rather less on the Edison board than on the Bell board, because, leaving out of question the employment of one operator on the former as against two on the latter, only one plug was used for each subscriber

connected on the Edison as against two on the Bell board; and Mr. Phillips. these plugs on the Edison board were very accessible, having merely to be removed from one position to another on the subscriber's vertical strip—that is, from the earth bar to the instrument bar. After ascertaining the number wanted, the operator removed the plug to the horizontal strip to be used in completing the connection. In practice, the operator using the Bell board kept the cords round her neck, and took them from there as required. She had to insert the round plug in the bar, and the flat one in the jack. These operations could not be performed so rapidly as those on the Edison board.

Mr. Kingsbury has described all these boards so well that I do not think there is much remaining to be said on the subject. With regard to the multiple board, I think Mr. Kingsbury is perfectly right in insisting on the importance of that development of the switch-board. There is not the slightest doubt that this is the great feature in the history of the telephone switch-board, which has enabled large numbers of subscribers to be dealt with. It is rather a difficult point for anyone to appreciate unless they have actually seen a switch-board working. If they can see a switch-board working, with six or seven thousand subscribers all within reach of one operator, they get an idea of the value of the multiple board which nothing else can give them. It would be impossible practically to work such an exchange with anything but that system now. Of course there are methods that have been adopted for dealing with exchanges up to, say, a thousand subscribers. There is, for instance, the ticket system. The girl getting a call writes down the number required and other particulars on a ticket, which she sends to another operator, possibly at the other end of the room, who has to do part of the work. But that is a cumbrous and slow method, and everything you can possibly do in a telephone exchange to save even the smallest amount of time is of very great value indeed. The telephone service is made up of very minute details, and the saving of a few seconds in connecting two subscribers, though it seems a small thing when considered by itself, when multiplied by the thousands of connections made in a day means

Mr. Phillips. a considerable gain. The operator is able to deal with more subscribers, and the service is more satisfactory in every way.

With regard to the multiple board, I would mention that in London we had a multiple board working, so far as my recollection serves me, before the Liverpool board. It was a board invented by my friend Mr. Hawes, who is here to-night, who at that time was in the service of the United Telephone Company; it was invented entirely independently of any knowledge of the American patent. The board was working for some two or three years in our Chancery Lane exchange. I may mention that it was rather peculiar in its construction, inasmuch as the jacks were placed vertically, instead of horizontally as usual. I may also mention incidentally that it formed a long table with a box-like top, and it was known in the company as the "Hawes trough." The introduction of the branching system is no doubt a very great step in advance of anything we have had before. It was, as Mr. Kingsbury says, a thing we had been trying to perfect; it supplied an acknowledged want, and it came very opportunely. Our Lime Street exchange, the largest in London, which is fitted with it, has been working since the first Monday in August, and, so far as we have had experience of the system, it works exceedingly well. It is, I believe, going to do us very good service. One point on which I differ from Mr. Kingsbury is where he says that the transition board, as he calls it, is a very practicable one. That is a point upon which, as Mr. Kingsbury knows, I and others have disagreed with him for some considerable time; and I think I may draw his attention to the fact that he says in an earlier portion of his paper that the leaving of an indicator as a leg on the line when the connection is made—which this board does—is an undesirable feature. I am very pleased to know that Mr. Kingsbury has arrived at that conclusion.

Mr. Hawes. Mr. F. B. O. HAWES: As I have some photographs of the multiple switch-board to which Mr. Phillips referred, and which was designed by me and used in the Chancery Lane exchange of the United Telephone Company, it may interest you to see them, more especially as it was the first switch-board of this description

at work in England.* [*The photographs were handed to the President.*] The reason for this board being fitted lying on its "back"—*i.e.*, the spring-jacks lying vertically, the face of the board being in a horizontal position—was that more operators could work at each section; it was arranged for two operators to work at each side of the table at each section, whereas, had it been fitted in an upright position, only two operators in all could have worked each section. This, of course, was a distinct advantage, as it reduced the number of sections necessary, and, consequently, the number of spring-jacks required. There were, however, serious disadvantages, principally owing to dust getting between the contact points, and the lines thus becoming insulated; also, the difficulty of making any repairs whilst the exchange was at work, as, in order to get at the connections, the board had to be lifted up, one side being hinged for that purpose. This position of boards has not, consequently, been generally used, and there are, to my knowledge, only one or two instances where the same position has been adopted. Mr. Hawes.

There are one or two other points I should like to refer to. In Mr. Kingsbury's valuable paper he refers to the different merits of the Edison (plug) board and the Bell (cord) board. Mr. Phillips has referred to this, and with what he said I quite agree. The "Edison" board could certainly be worked quicker than the "Bell" board; in addition to which, the latter gave much trouble owing to the difficulty of obtaining good cords: those we had were continually breaking.

They were made of fine stranded wire instead of the tinsel which is now used. As the cords are such very important items in the working of a multiple switch-board, I think it might be interesting if Mr. Kingsbury would refer in his reply to the present form of cords.

The correction which I think it desirable to make is with reference to the statement made by the author as to the

* It may be of interest historically to know the exact date when the first multiple switch-board to my design was fitted up by the United Telephone Company. I find from notes that on the 20th November, 1883, the central trunk exchange at Oxford Court, Cannon Street, was opened with this pattern of board in use. The Chancery Lane exchange was opened a few weeks after this.

Mr. Hawes. Liverpool multiple switch-board being the first used in England; the multiple switch-board being in use several years before that.

With regard to the details of the "branching system," which Mr. Kingsbury has referred to—the description of which is not printed in the proofs which we have before us—I think that this contains many of the greatest advances that have been made in switch-boards, and would have been glad if we had more details as to its carrying out and working.

Mr. Calder. Mr. A. CALDER: I have nothing to add to what Mr. Phillips has said. It has been a very great pleasure to me to listen to Mr. Kingsbury's paper. I have had much experience with switch-boards for many years.

I understood Mr. Kingsbury to say that the diagram on the wall was a photograph, and is, I believe, the board upon which I worked as an operator when I entered the service of the company.

I know, of course, the Edison board very well, and I can only corroborate what Mr. Phillips has already said—that Mr. Kingsbury has dealt with the subject so particularly well, that, as far as I can see, there are really no debatable points in it.

I also agree with Mr. Phillips that if there is one man in the United Kingdom who is better able than another to read a paper on the origin and development of the switch-board, it is Mr. Kingsbury, of the Western Electric Company.

Mr. Addenbrooke. Mr. ADDENBROOKE: I am afraid there is really little I can offer in the way of discussion. The paper is a historical one, and I must join with the other speakers in acknowledging the exceedingly lucid way in which it is put together. I can recall experiences of the early days, and I remember that board in Coleman Street, and the Edison board at Queen Victoria Street. Also another form of board which I do not think has been referred to, but which we used in a modified form for electric lighting now. It was in use for the Stock Exchange centre; it consisted of a series of strips with holes in them, and another set of strips laid underneath; plugs split longitudinally were pushed through the top hole and into the bottom, making a spring contact, and joining the two lines together in that way. Of course

Mr. Addenbrooke.

all these boards have gradually been swept away. I myself made one or two attempts to design boards, but there is no doubt that English telephonists have been enormously handicapped in comparison with their American brethren. Unfortunately, in the early days of the Telephone Company the telephone authorities seemed to consider that, so far as their staff was concerned, the finality of things had come, and that if any improvements were to be made they should come from outside. I do not think, from what I recollect, that there was any want of ability amongst the *employés* of the company at that time—in fact, what has been said with regard to the Hawes board has shown it; but certainly at that time any work of an inventive or ingenious kind was systematically discouraged. On the other hand, I think it is desirable that people should know generally how ably the Western Electric Company have acted in this matter. I myself was in America a little over four years ago, and I had the pleasure of an introduction from Mr. Kingsbury to Mr. Barton, the president of the company, who had me shown over their works at Chicago, and explained their system of work. They have always had a special department dealing with improvements and inventions, and that department has been in communication with the telephone companies, as far as possible, throughout the country, and in receipt of information from them concerning complaints and everything dealing with the subject. It was the duty of this department to go through the mass of complaints and suggestions of one sort and another, and to see how the faults could be remedied, and how the suggestions could be put into practicable form. There is no doubt that this has been a most successful policy. At the present day, when we look back on what has been done in the way of telephone switch-boards, we may almost say that the progress of telephony has rested with the Western Electric Company and the affiliated companies, and the various officials and district superintendents who, from knowledge of the practical working of the exchanges, have been able to make suggestions from time to time. I must say that when I was in the Telephone Company it was a source of great sorrow to me to see that English electricians had not the chance that

Mr. Addenbrooke. Americans had. Probably matters are improved in this respect now, but I do think that English people should take a leaf out of the book of the Western Electric Company. That company's methods have been exceedingly successful financially, and they have managed their business with satisfaction to their staff and to the general contentment of everybody, and at the same time they have secured a class of plant which has remained unsurpassed. I have had occasion to act as consulting engineer for companies requiring switch-boards on two or three occasions since I left the Telephone Company, and have therefore been able to keep myself in touch with the work; and anyone who knows anything of mechanics must admire immensely the beautiful work which is put into those boards. The amount of detail is immense, and the way in which all the difficulties are overcome, together with the simplicity of the different parts, make an up-to-date switch-board one of the finest applications of mechanics which we have. I do not think Mr. Kingsbury has really brought out as far as might have been done the intense complexity, yet simplicity, of a multiple board. Say you have an exchange of four or five thousand subscribers, there are a considerable number of connections to each subscriber, and therefore the number of multiple cables which have to be employed in fitting up such a switch-board is simply immense. At the same time, everything must not only be made right, but made so that it will stay right. When I was in the company I do not think we had got beyond an exchange of 1,000 subscribers, and I myself was connected more with the overhead work than the exchange work, but I know what a difficulty it was to move the overhead work from one exchange to another with four or five hundred subscribers.

I hope that this paper will be merely a beginning. There is not much to debate about in what Mr. Kingsbury has said, but it lays a foundation on which a mass of interesting material might be built. Particularly, I think somebody should say something about the exchange system of London. For two years I had charge of the testing and trunk line department, and this system was brought very much under my observation just at

the time when it was developing; in fact, I can remember being present at the meeting at which practically the central trunk system, which was adopted, was decided upon. There is no doubt that London presents a different telephone problem to that of any other city, as it is so very much larger; and the London companies have had to deal with a state of affairs which has been met with neither in Paris, New York, Boston, nor any other great city. It so happened when I was there we used gutta-percha cables, therefore we had considerable statical capacity. It was exceedingly difficult to get below that capacity, and you could not work above it. Towns like Paris or New York were not sufficiently large to introduce very great difficulties in putting in the lengths of cable which were necessary to reach out to what I may call the less dense areas. What I mean is this: As you get nearer the exchange, your wires get thicker, and there is no room for overhead wires; but in London we had many centres—there were centres in South Kensington, Paddington, and so on—a sort of nuclei—all of which had to be connected together by trunk lines. Therefore we had to carry a great mass of trunk lines from one end of London to another, sometimes three or four miles in length, and even more than that, and it is a most difficult question to know how to do it—or it was in my day. There is no doubt it has been simplified since by the introduction of cables of lower capacity, and by the use of metallic circuits. In my time the capacity was such that when we got beyond two or three miles of cable the conversation was certainly very deplorable; we were always having complaints about it, while to correct it did not seem possible without recourse to doubled wires. We had, therefore, to meet a problem which, as I say, I do not think had been met in the same way anywhere else. The Central Trunk Exchange was decided upon to meet it, which is very much like what the head is to the body. You know there are in parts of the body certain ganglia which control the local muscular movements, and just in the same way we had exchanges established. I think there were 22 different exchanges in London at one time—a greater number than in any other city. These had all to be connected so that any one exchange could communicate with any other through

Mr. Addenbrooke.

Mr. Addenbrooke. the Central Trunk Exchange, because it was obviously impossible to carry sufficient trunks from one exchange, say in the South of London, to all the exchanges in the North or East of London. It is thus necessary for almost every call to go through three exchanges—two terminal exchanges and the central—since in London by far the greater proportion of calls are on the trunk system. In this way a problem was introduced in London which had not to be dealt with to the same extent on any large scale in any other city; because, besides the extra complications, you have the time occupied by the call going through the Trunk Exchange, in addition to the time occupied at the exchanges at the ends of the trunk lines, instead of all the work being done in one exchange. This problem was met in London in my day, and I know that great improvements have been introduced since.

I think, therefore, if we could have a paper which would deal with this question from somebody who is thoroughly up to date, that it would form a most interesting contribution, and would at the same time show that English telephonists are not altogether playing a secondary part.

Note.—In what I have said above I allude solely to the work of the Telephone Company, and not to the admirable work on long-distance circuits, &c., by the English Post Office, which has gone far to remove the reproach that English electricians have done nothing appreciable in telephony.

Mr. Clay. Mr. C. B. Clay: As this is rather a historical paper, I should like to say there is one switch-board, or, rather, one modification of one of the switch-boards which Mr. Kingsbury had dwelt upon, which he has omitted to mention. I refer to the Bell board. The Bell board was subsequently worked to a very large extent in England. It was used at the largest exchange in Liverpool. There was a slight modification. The spring-jacks were altered, and the upper strips entirely done away with. The switching was done direct by cord from one spring-jack to another. It may also be interesting to notice—I can remember, somewhere about 1882, I think, as far as England is concerned—it was pretty generally agreed that any switch-board which introduced a cord was practically inadmissible for telephone business, on account of the great

difficulty of keeping the cords in order. Mr. Kingsbury has unfortunately shown us most of our good things have come from America, but, if I may say so, it is only that America has been a little bit in front of us. In the first case there is the multiple board of Mr. Hawes. That, of course, we should not now be working in the form which then existed; but the germ was there, and I daresay it was quite as good a board as the original multiple board started in America. There is one other point, coming now to the branch system. I know before we had it from the Western Electric Company the branch system was designed by Mr. Watts, of Newcastle. He was unfortunate in being a little bit behind the times, but in this case very slightly. Mr. Clay.

Mr. W. AITKEN: We have listened with very great interest to the historical paper on the origin and development of the switch-board, but I am sure that we must all be very sorry that Mr. Kingsbury stopped where he did. They have in America the divided switch-board and the visual signal arrangement, that we should like to have heard something about. Mr. Kingsbury lays claim to one or two matters as to which I respectfully differ. He associates the year 1894 with the use of a ring on the front of the spring-jack for a separate test. That insulated ring is not new, but is at least three years old. It was used in this country experimentally, and used extensively in Stockholm about the time referred to. Again, Mr. Kingsbury spoke of spring-jacks as being successfully reduced. His company have reduced from 15 in. × 5 in. per 100 subscribers' jacks to 11½ in. × 2½ in.; another manufacturer has still further reduced this to 9½ in. × 2⅛ in., so as to put the subscribers still more within the operator's reach. The branching system of wiring boards is not new. Members here might naturally think the branching system was introduced with the self-restoring indicator described by Mr. Kingsbury; but the branching system has been known, and successfully worked, for a number of years in this country. The flat board has been mentioned. This is being introduced considerably in London. The advantages, of course, are that with half the number of spring-jacks you can work the same number of subscribers as with an upright board. These being on a table- Mr. Aitken.

Mr. Aitken. like surface, the operators work from both sides. That means a very considerable saving in first cost, and in a given space you can work a very much larger number of subscribers—nearly double the number. As space is very valuable in London, this is a serious consideration.

An improvement in Scribner's single-cord board, overcoming the necessity for the extra movement in returning the plug to the socket, has been made, and there are now boards very successfully working on the single-cord system (I might cite as examples two new exchanges in Stockholm, and one now being built in Copenhagen); and where a listening key is in each cord it is a very much speedier operation, and, as only half the number of cords are in use, a great saving in maintenance is effected.

In further reference to the branching system of the Western Electric Company, in each exchange there are three shunts across the circuit. This, with the particular indicators used, is perhaps not a very serious thing; but other manufacturers have taken steps to cut two of the three out of circuit, leaving only the ring-off in shunt.

There are many things that might have been mentioned in regard to the accessories and methods of working switch-boards that British telephonists can claim to have originated or improved which time does not permit me to dwell upon.

Mr. Lorrain. Mr. J. G. Lorrain: I concur most heartily in the praise which has been accorded to Mr. Kingsbury for his paper. It is one of the best we have ever had on the subject of telephony, and we may hope that we shall have a great deal more upon this subject. On the question of switching arrangements, I think any historical retrospect would be lacking if no reference were made to what was done before the advent of the telephone. In Glasgow (and I think also in Newcastle) there was a very large exchange worked by means of the Wheatstone A B C instrument. My recollection of the Glasgow exchange is that there were above 120 subscribers joined up, and for an A B C instrument exchange that is very large considering the trouble of working.

With regard to these early boards, I am rather astonished to

hear that board spoken of as the Bell board. At the time we used these—and I erected the first one—it was known as the "Williams standard switch-board," from the fact of its being manufactured by Charles Williams, of Boston; and I believe also that it was his invention. You could get 75 exchange lines on it, but it required two operators. Mr. Lorrain.

There are one or two points in Mr. Kingsbury's paper that I might refer to. He seems to think that the first exchange started in this country was the one in Coleman Street, London. As a matter of fact, the Manchester exchange, started by me in July, 1879, was about a week or a fortnight before it; then came the London exchange; then, I think, the Paris exchange; and then the Liverpool, Sheffield, and Wolverhampton exchanges in quick succession.

Mr. Kingsbury speaks of the connecting plugs originally used in Manchester and Sheffield sliding over a rod with notches for each horizontal bar. This was not used in Manchester originally, although it may have been used there at a subsequent period.

Mr. Kingsbury says: "The multiple board as originally "designed employed the Scribner 'jack-knife' switch, which was "never used in England, I think." I rather think that in the summer of 1880 either Mr. Scribner or Mr. E. M. Boston brought over a model of that, and I think I remember testing it at Clark, Muirhead, & Co.'s at that period.

Now I come to the point about the metallic circuit switch-boards. Mr. Kingsbury said: "It is possible that some may "be surprised to hear that metallic circuit multiple boards are pro-"vided for in Scribner's 1879 patent;" but really that is not surprising when we bear in mind the fact that Bell and others were working hard at that period—the latter end of 1878 and the beginning of 1879—on metallic circuits.

This recalls to my mind a point upon which I would like to get some information. I refer to the double twisted wire arrangement, such as we have on the lines between London and St. Margaret's Bay for the French telephone cable. This metallic circuit arrangement was spoken of for a long time as the "Post "Office system," and I believe in all the text-books the invention

Mr. Lorrain. of this was referred to the Post Office till recently. Then it was referred to as the invention of Moseley, Bottomley, and Heys, who patented it in July, 1880; but, as a matter of fact, the invention dates much earlier. I erected a number of exchange lines on that system in September or October, 1879, in Edinburgh; but I got the idea from Mr. J. D. Tracy, an American, who explained to me that lines were erected on this plan in some American exchange with which he had been connected. At any rate, it is quite clear that this way of crossing wires was in existence in America at least early in 1879, and probably before that. I do not know where the invention came from, but it is always wrongly described in the existing text-books.

I am rather sorry Mr. Kingsbury has not mentioned some of the other switch-boards (for example, Jones's switch-tables, used in the Scottish and Belfast exchanges), as in properly tracing out the development of the exchange switching arrangements it is necessary to bear in mind the large number of other boards that have been brought out by other men. I should have liked to have some reference to the "Law" system, and "Mann's" modification of it. I cannot help thinking that there is more in that (the "Law") system than in the existing system; and I think development will bring us rather to the lines of the "Law" system than to those of the Western Electric system. During a recent visit to the United States I had an opportunity of seeing two typical exchange systems—one the new system being erected by the Western Electric Company at Cincinnati, where they have put up, I believe, the biggest multiple switch-board ever erected, and the other the "Law" system as carried out at St. Louis; and for excellence I have never seen any exchange work that, in my opinion, came within measurable distance of that of the system at St. Louis—a system on the old "Law" plan, but, doubtless, with modifications and recent improvements.

Mr. Holmes. Mr. PHILIP HOLMES: I may mention, in connection with Mr. Lorrain's remarks in reference to the twisted wire system, that Mr. W. F. Bottomley, in conjunction with Mr. Charles Moseley, both of Manchester, were, I believe, the first to start a telephone exchange in the United Kingdom, and this was in Manchester.

Mr. W. F. Bottomley, who was the late superintending engineer of the National Telephone Co., Limited, before its amalgamation with the United Telephone Company and Lancashire and Cheshire Telephonic Exchange Company, on more than one occasion informed me that he, in conjunction with Mr. Moseley, had taken out a patent for the twist system; but even in those days—about 1880—the idea as to utility was almost discredited, and when before his death he found telephone companies, as well as telegraph companies, using the system, he felt that the inventors were not getting the credit or recompense which they deserved. Mr. Holmes.

I have not searched the Patent Office, but I should imagine that Mr. Bottomley lost his right over the patent by not keeping the same in force. This could be readily seen by reference.

The PRESIDENT: I must close this discussion, so that Mr. Kingsbury can now reply. As I expected, from the condition of affairs which at present prevails in England in telephone matters, the discussion has been very limited in its nature. There has been little criticism except from one speaker, and I agree with him that I do not think English electrical engineers have reason to be very proud of the telephone service in England. I know that this is not the fault of the engineers themselves, but is a political question, as there are other causes at work; but the fact remains that, if we users of telephones compare our English experience with our experience of some of the large exchanges in Continental towns, we find that the comparison is unfavourable to England. In many Continental countries the telephone has gone ahead so far that it is the *necessity* of the many, whereas in England it remains the *luxury* of the few. It is this that has confined the discussion this evening within such very narrow limits—in fact, to the very few firms who are dealing with telephone business in the United Kingdom. I hope, in the interests of telephone engineering generally, that we shall soon see this state of things altered. The President.

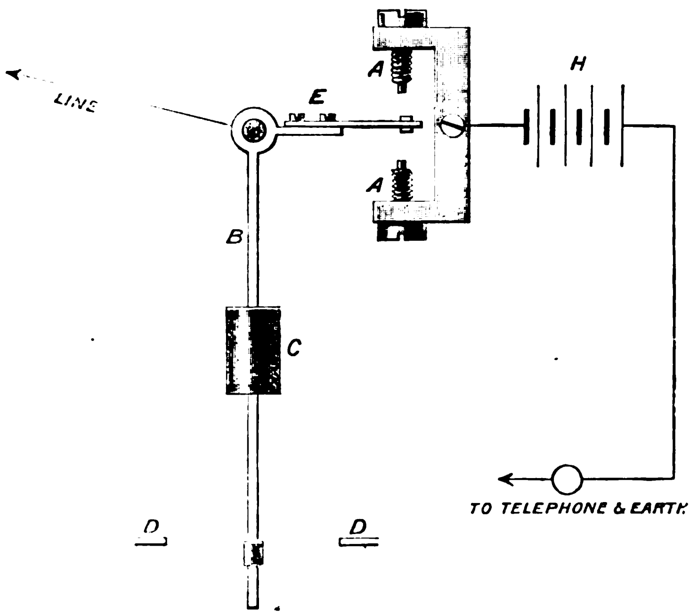
I regret also that so little has been said on the subject of the system of trunk lines which connect the various London exchanges, and in which lie—we are always told—the great difficulties of the London telephone service.

Mr. Latimer.

Mr. F. D. LATIMER [*communicated*]: In cases where it is required to work a number of telephones to a central point, and it is also desirable to economise both with regard to the amount of wire used and the expense of erecting and maintaining it, the following is a good system of working them all in series on a single wire; and, as far as I am aware, the only method without using complex mechanism. The credit of the invention is due, I believe, to Mr. Brown, of the Eastern Telegraph Company.

The central point is enabled to speak to any desired instrument by the following arrangement, which, I should say, is only applicable to instruments in which the ringing is effected by battery power and not magnetos :—

At the central telephone is fixed in the line an apparatus as shown in Fig. 1. A steel rod, B, capable of swinging between the



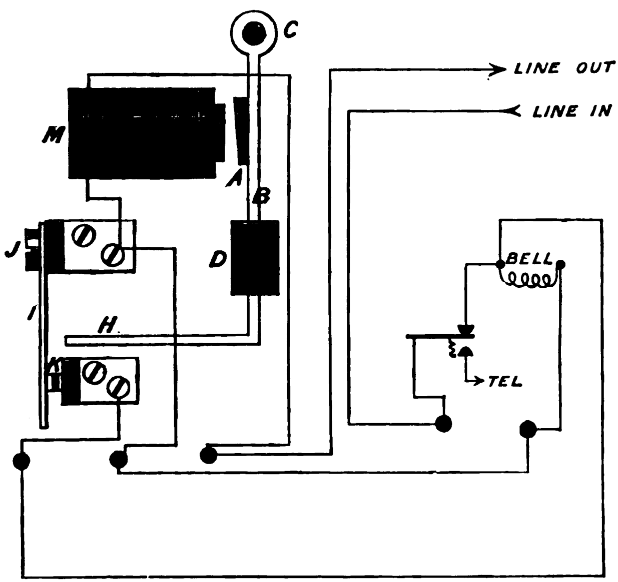
stops D D, has on it a weight, C, which can be moved up and down; at the top of this rod B, and at right angles to it, is placed a metal arm, E, provided with contact points at its extremity.

It will now be readily seen that when the rod B is set

swinging, a number of currents will be intermittently sent to line Mr Latimer. from the battery, H, the circuit being closed by the contacts on E touching A A.

By altering the position of C, it will be understood that the number of contacts made, and consequently the number of currents sent to line, can be increased or decreased, according to whether it is moved up or down.

Now at each of the other instruments is placed, in series with the telephone, the apparatus in Fig. 2. M is an ordinary



electro-magnet, and A its armature, fixed on a rod, B, pivoted at C; on this rod is fixed a weight, D (the weight occupying a different position at each telephone). Now, when any particular number of currents in a given time are sent by depressing the key at the central point (the operator, of course, first adjusting the weight to the correct position and swinging it), they travel through the magnets, causing their respective armatures to be rapidly attracted and released; but at only one of these instruments (viz., that one at which the weight is adjusted to suit that particular number of currents) will the arm, H, swing, sufficiently to push the

Mr. Latimer. spring, I (held by a screw at J), away from the contact, K, causing a disconnection in that branch of the circuit, and thus compelling the currents to travel through the comparatively high resistance of the bell coils, and thereby ring it.

Thus it will be seen that the central point is enabled to speak to any one instrument by adjusting his weight to a certain point, corresponding to that particular instrument.

Mr. Gill. Mr. FRANK GILL [*communicated*]: In common with others, I have to express my gratitude to Mr. Kingsbury for the very valuable paper he has presented, and I am quite in accord with Mr. Phillips in his remarks as to Mr. Kingsbury's ability for the task.

With regard to the simile in the paper of making two blades of grass grow where only one was before, there is a point which appears to have been missed by the speakers. While undoubtedly true that two operations of the Edison board—that of breaking a line, and making another connection—were in the Bell board condensed into one motion, a further movement was added. The movements in this part of the work of making a connection on the Edison board were two—1st, taking the plug out of the earth bar; 2nd, inserting the plug in the instrument bar. On the other hand, the movements in the Bell board, for the same work, were also two—1st, inserting the plug attached to a cord into the line jack; 2nd, inserting another plug (which, being a thin slip of metal, was generally termed a "knife") into the same jack: this knife was connected with the operator's instrument.

Mr. Kingsbury mentions the first introduction of cylindrical plugs. I fear that the advantages of these are even yet not thoroughly appreciated (I am speaking of ordinary boards, not of special trunk switch-boards), from the adoption of a flat plug, and one which, moreover, has to be used one way upwards, in a recently built exchange which is sometimes cited as a model. Those who have had a working experience of a large and busy exchange know the value of even a small fraction of a second, and the speed which it is possible to attain upon a modern switch-board is almost entirely attained by strict attention to what may appear trifling details, each of which saves perhaps only a very

small part of a second of time. Among these details I do not, of course, refer to the multiple system, the advent of which marked a distinct epoch in the history of telephone switch-boards. Mr. Gill.

The reference to the Liverpool boards is not quite correct. Although there were no answering jacks, the lines answered on each board were arranged so as to bring the jacks as close as possible to the operator attending them. For instance, the first board accommodated those lines whose numbers lay between 1 and 100, and also between 301 and 400; and the block of jacks 1–100 were, of course, situated at the left of each board, and were thus within reach of the first and second operators on the board. The numbers 301–400 were situated towards the right-hand side (there being five panels, and the sections of 100 running consecutively), and were thus accessible by the second and third operators. The use of answering jacks is certainly of great advantage, as they not only bring lines within easy reach of the operator, but also divide the cords, so that the line jacks are not so crowded.

With the reduction in the size of the jacks came another constructive alteration. The springs of the jacks and other apparatus used to be of phosphor-bronze; now they are of German silver.

There is another alteration which is interesting historically, the date of which I am unable to fix: this was the adoption of cords fixed in place in the boards, with weights to keep them in tension and to keep the plug in contact with a plate which was generally an earth connection.

Mr. Kingsbury and some of the speakers have referred to the complexity of a telephone switch-board; and those not personally acquainted with one will perhaps better understand the nature of the problem which has to be met if I say that it is sometimes necessary to deal with over 45,000 calls per day on an exchange of 3,000 lines, and that many of these calls will have to pass through more than one exchange; also, that in an exchange of the size mentioned, arranged as at present general, there would probably be at least 58,800 jacks to be kept in order, besides movable contacts in the shape of springs to the number of about

Mr. Gill. 12,700 in the key-board apparatus alone. It would be difficult to find among the other applications of electricity to everyday life a piece of machinery so complex.

Mr. Kingsbury Mr. KINGSBURY: I hope, Sir, that I shall not be out of order in replying to the various speakers in the reverse order to that in which they have spoken, and so commencing with yourself. I think we may regard telephone subscribers as fairly average men; they are fathers of families— some of them—and, like fathers, they differ in their views. Some fond parents consider that their own children are paragons, whilst others are unable to pay an occasional call without forming and retailing the opinion that other people's children behave very much better than their own. This is very much the case with telephone subscribers. Those of one town, especially if that town has rather a peculiar system (like St. Louis, for instance), think that there is no other system like theirs; whilst others can do nothing but find fault with that which they find close at home. Speaking as one who has devoted some attention to the subject, I may express my belief that, if the difficulties of working the London telephone system were more generally known, greater consideration would be shown for those who have to work it. I speak generally, and without reference to trunk lines or other details. This is one of the points mentioned outside the strict subject of my paper, and it is not my intention in replying to discuss such features, although very interesting in themselves. I should be glad to have my say upon them, but must confine myself to the points raised in reference to telephone switch-boards.

I am very glad that my paper has been the means of eliciting from one of the early workers in the telephone field like Mr. Lorrain remarks of such interest. I presume he is right in considering the "Bell board" as of Williams's manufacture. I do not know this as a fact, but infer it from the design of the indicator. The Williams board which went into most general use was, however, a plug board. In adopting the name of "Bell "board" I was guided by the fact that it was generally known as such here, and I have, wherever possible, taken local examples and illustrations. In this instance, however, I think that to have

used the name by which it was originally known would have been confusing, because there must be hundreds of Williams plug boards in use for one cord board. The board with plugs sliding over rods was another Wiliams type. I have seen them in both Manchester and Sheffield, but I did not wish to imply that they were the first kind used at either place. Mr. Lorrain can speak from experience as to Manchester. I should like to have had time to mention other boards, but I had to make a selection of types, and preferably selected those originally used in London. I sought to give the principles rather than the details. The Jones switch was a plug switch of the upright bar and cross bar type. Its special feature was a key-shaped plug with a spiral spring at the handle end, passing through a hole of like form in the front bar, so that a half-turn locked it in its place by the pressure of the spring. In this respect there was some advantage over the loose plug, but the principle was the same as in what I have called (only because it was known here under that name) the "Edison board." The same remark will apply to most of the variations in design or name. Mr. Lorrain will note that the title of my paper will not permit me to enter into a discussion of systems, however tempting I might feel such a subject to be, but his reference to a Western Electric system might be somewhat misunderstood. The Law system is quite as much a Western Electric system as the indicator calling system. I do not know whether Mr. Lorrain is right in stating that Scribner's board was exhibited at Clark, Muirhead's, in 1880. It was exhibited at the Paris Electrical Exhibition in 1881, and shown at Clark, Muirhead's, early in 1882. Mr. Kingsbury.

Mr. Aitken regrets that I stopped where I did; but, considering the length of my paper, I think Mr. Aitken must be alone in feeling that regret. Undoubtedly the more novel features which he enumerates, and some others which he does not, would be of greater interest to himself and others who are thoroughly familiar with the types to which I have referred. Had I considered my paper from that point of view, I should have opened the book at the end, but, recognising that it was an introductory paper, I opened it at the beginning, and did not finish it by any means.

Mr. Kingsbury.

Mr. Aitken also objected to my omitting several improvements. If I had included all the improvements, I should have kept you here till next week, but I should not then have mentioned either the single-cord board or the flat board. The flat board I think I may put to electric lighting men in this way: There is an engine which you may be able to get at a low price, but which costs you an expensive amount in fitting and in fuel. You do not regard that as a satisfactory engine. That is the flat board, in my opinion, and, I think, on sound evidence. The single-cord multiple board is one of the inventions with which Scribner's name is more immediately identified here, as it has been described in English text-books under Scribner's name, whereas his other inventions have gone under the Western Electric Co.'s name. This board, although promising extremely well when first introduced, has been found to have disadvantages, and is not in the line of development. Therefore it was not included among the types brought before you. As to the great saving effected in maintenance of cords alleged by Mr. Aitken for the single-cord board, it must be remembered that, although only half the number of cords are in use *when connections are made*, double the number are used *for the purpose of making connections*; and this being considered may possibly put a different complexion on the question of saving.

Mr. Aitken tells us that an improvement has been made in Scribner's single-cord board. Of this I have no personal knowledge, and will therefore keep an open mind upon it until Mr. Aitken or some other member brings the matter before us in such a way that we may discuss it, and arrive at conclusions of our own as to the improvement or otherwise; but, whilst keeping this open mind, I see no objection to my saying that in other cases which have come before me where Scribner had been "improved" by other manufacturers of necessarily less experience, I have generally found that the improvement is a somewhat superficial one, at the expense of some feature of fundamental importance. The manual replacement of the plug in the single-cord multiple board was very deliberately decided on in spite of the obvious advantage of its omission.

I associate the year 1894 with the ring on the front of the spring-jack only because of its use in the branching system; and, as the branching system was introduced in London in 1894, this is strictly in pursuance of my plan of giving you local examples wherever possible. That is the only way in which the year 1894 is at all associated in the matter. I did not mention that ring with any intention of claiming originality for it, but simply referred to it as an instance of evolution. Just as in nature a tail remains where a tail is needed, and is discarded where it is not needed, so the ring in the front of the spring-jack is retained for testing purposes in the same position and form as in its earlier uses, when it was something more than a test ring. It is now simply an outlying branch for test purposes only. As I have said, I had no intention of claiming any originality; but since Mr. Aitken raises that point I am bound to say that I know of no other system, experimental or practical, in which a similar contrivance is used, and I think that Mr. Aitken has altogether misunderstood the drift of my observations on that point. Again, I am afraid that Mr. Aitken has misunderstood me on the size of the jacks. He tells us that whilst my company has reduced them from 15 in. × 5 in. per 100 to 11½ in. × 2½ in., another manufacturer has still further reduced them to 9½ in. × 2⅛ in. In stating that my company has reduced the size of the spring-jacks to the dimensions given for those of another manufacturer, and that I have samples measuring 8 in. × 1⅜ in. per 100, I wish particularly to avoid giving the impression that I see any special merit in reducing the size of spring-jacks, unless there is some feature in the design which makes practicable what was not practicable before. With a given model, there are few manufacturers who would find any particular difficulty in reducing jacks to microscopical proportions; but it is not only a question of manufacture, but also of use,—not only a question of jacks, but also of plugs. Nor must the cabling be overlooked. In referring at all to size of jacks, I simply desired to show that, in so far as the material is concerned, it is possible to increase the capacity of a multiple board beyond any requirements hitherto indicated as likely to be necessary. I gave no figures,

Mr. Kingsbury

Mr. Kingsbury. but contented myself with saying that the size of the jack had been repeatedly reduced. The ultimate sizes may never be needed, but if needed they are ready.

On the branching system Mr. Aitken fears that you might think it was only introduced with the self-restoring indicator described by me. I do not think there is any danger of your arriving at such an impression, because in my paper I distinctly told you the contrary. In the branching system of the Western Electric Co. he tells us there are three shunts across the circuit, which with the particular indicators used is not a very serious thing; but other manufacturers have taken steps to cut two of the three out of the circuit, leaving only the clearing-out indicator in shunt. Mr. Aitken does not remember my explaining to him that the Western Electric had already done what other manufacturers have taken steps to do; or my showing him Specification No. 3831 (1893), which describes the manner in which the subscribers' drops, besides automatically restoring their shutters, also automatically cut themselves out of the circuit. This device is not needed at present, and we do not know that it will ever be needed. So long as the drops remain harmless in the bridge, so long will it be better to keep them with permanent contacts to the lines, instead of adopting the cutting-out system, which at present is only necessary with imperfect indicators; but the method was introduced by the Western Electric Co., and is ready, together with a great many other ideas, in case the practical working of exchanges should render its use advisable.

I have naturally a good deal of sympathy with Mr. Clay's remarks about our good things coming from America, only because America is a little in front of us in point of time. Mr. Hawes was unfortunate in independently inventing in 1883 what had been previously invented in 1879. Mr. Hawes mentions the Central Trunk Exchange as the first, but he applied the same principle to the junction lines in Coleman Street a few months earlier. So far as Mr. Watts's design is concerned, I do not think there is any question of priority at all. If any question exists, it is one of practicability. Mr. Watts's design for a branching system must be one of many, not only here, but "over there,"

Mr. Kingsbury.

where all the good things are said to come from. You will observe what I said about the idea of the branching systems being "in "the air;" I might have said there were numbers of such ideas "on paper." I do not doubt that each one of those designs had some merit, but all lacked the necessary element of practicability; and I am sure that Mr. Watts, like most others striving for the same ends, would be the first to recognise the simple and practicable nature of the system which I have mentioned as *the* branching system, but which I am quite content to define as *a* branching system if the indefinite article should be preferred.

Mr. Addenbrooke has been too complimentary in his observations generally to call for a reply on my part, but he has alluded to the fact that I did not bring out as fully as might have been done the immensity of the work involved in fitting up some of the large switch-boards used. That is quite true. But I did not forget that point. In the last few lines of my paper I referred to the setting up and keeping in order as no easy task. Had time permitted, I should have liked to enlarge on that subject, for I believe that very few realise the amount of detail to be gone through, the care to be exercised, and the responsibility incurred in fitting a large central office.

Mr. Hawes asked me to refer to the present form of cords. The switch cords now differ very considerably from those originally used, being better conductors, and very much stronger, whilst retaining their flexibility. The improvements have been considerable; but if Mr. Hawes had only remained in the telephone world, as he ought, I believe he would still regard cords as necessary evils. Notwithstanding the care expended upon them, there is still room for improvement, and perhaps a promising field for new departures; but if anybody should have a bright idea on the subject, I would suggest his not crying "Eureka!" until his cords have been in actual use for several months in a busy exchange.

I think I must ascribe to my inexperience in the preparation of scientific papers an apparent difference of opinion between Mr. Phillips and myself. If I had only left the blades of grass in

Mr. Kingsbury.

their proper place, they would not have given rise to a misunderstanding. I agree fully with all that Mr. Phillips has said about the advantage of one operator doing all the work of a connection, instead of being dependent on another. My simile had reference only to removing the earth and connecting to the line. Mr. Phillips, Mr. Hawes, and Mr. Gill have understood—and from the nature of the simile and the brevity of the description very reasonably understood—that I referred to the comparative working of the two boards. I have no fault whatever to find with what Mr. Phillips says on the transition board. Still I have not to alter the language of my paper. The transition board is not theoretically perfect, but experience has shown that it is "very practicable."

Mr. Latimer's communication relates to a class of apparatus which to some extent preceded switch-boards, a number being invented immediately upon the introduction of the telephone. They were termed "individual bells," and used on "party lines," but the result of their working was in most cases not so satisfactory as to lead to their continuance in use. "A keen observer of the signs "of the telephonic times," however, considers it evident that a return to party lines in some form will by-and-by be inevitable. I refer to Mr. T. D. Lockwood, who read a paper (printed in vol. ix. of the *Transactions of the American Institute of Electrical Engineers*) entitled, "Selective or Individual Signals," in which he summarises the principles underlying the various inventions of this kind. Of "bells working on the principles of harmonic "telegraphy," where, "at each station the spring, pendulum, or "reed carrying the bell hammer is tuned to respond to a different "note or rate of vibration," he says: "Operated always by "experts, and carefully constructed and adjusted, these might "do well. They are, however, too sensitive, tend to become "operative as their proper number of vibrations is approached, "and in some degree become irregular with temperature "changes." It will be understood that these remarks are made of a type for the guidance of new inventors, and do not refer to the particular apparatus described by Mr. Latimer, which has undoubtedly been designed with a thorough knowledge of the

principles governing the action of such apparatus, with a view "to produce a very certain and efficient electric signal." (Vide *Electrical Review*, vol. xv., p. 125.) Mr. Kingsbury.

I have to thank Mr. Gill for his correction about the Liverpool boards. I had forgotten that those boards were so arranged as to partly overcome the difficulties existing prior to the introduction of answering jacks. I have referred to some papers on the subject, and find a memorandum showing the numbering of the jacks and indicators as I believe they were adopted on the first Liverpool boards. The jacks were numbered consecutively, as described by Mr. Gill. The indicators were as follows (0 standing for the first hundred, and so on):—

2·8	0·3	5·7	6·9	1·4

This arrangement made it unnecessary for operators at one section to reach two adjacent blocks of jacks, but the concentration of cords remained; and, as Mr. Gill remarks, the provision of answering jacks not only brought the lines within easier reach of the operator, but also reduced the cord tangle.

I think that the use of German silver line springs on the jacks preceded the reduction in size.

I do not know at what date, or by whom, the pulleys and weights were introduced. The first illustration of them I have as yet seen is in a United States patent on the standard board (No. 247199), filed April 4, 1881. The arrangement of the plugs and the pulleys there shown is practically the same as used now, but the specification says "the flexible cords may be held taut *in* "*the usual manner* by weights." A United States specification of earlier date (No. 230069), filed May 14, 1880, shows the plugs arranged in order overhead, and the cords wound on spring reels. The superiority of the simple pulley weights, and their continued use, will be readily understood.

Having replied to the remarks of the various speakers in detail, I may be permitted, perhaps, to say a few words having reference to the fact, which has been alluded to, that the apparatus I have brought before your notice is mainly the work of our American cousins. You must remember that the telephone was born in America, bred in America, and brought up in

Mr. Kingsbury.

America. Its use has been more fully developed there. The problems that telephonists have to meet arise there first, are systematically dealt with, and solved as promptly as possible. We have no occasion whatever to grudge America its success in an industry of which it is the home, when we consider that for the same reason we have so many successes of our own. It goes without saying that there is no want of inventive ingenuity or originality in working methods amongst English telephonists or those of other nationalities. It has happened in several cases, to my knowledge, that devices which have been worked out here because experience has shown them to be desirable have already been in existence because similar experience has come first elsewhere, and for the reasons already mentioned such devices have been developed and brought into practical shape in advance of local requirements. Yet there is something to be learnt everywhere, and those who have to provide for the rapid development of telephone work cannot disregard general experience; in fact, it is not a matter of opinion, but is capable of ample proof, that the patriotism which spells prejudice is a distinct bar to telephonic progress. The time has long gone by when it was considered that a telephone engineer ought to evolve a switch-board from his own brain and have it made up at his own bench. The work has become specialised, to the immense advantage of the telephone industry everywhere. Although I have given you the names of some of those who originated certain features, I would not have it overlooked that a completed design is really the corporate work of specialists in each department, such work being carefully organised, with a result which would be impossible with individual or local effort.

It only remains for me to thank you, Mr. President and gentlemen, for the patience which you have shown during the reading of my paper, and those who have spoken for the kind remarks which they have made.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Member:

Horace F. Parshall.

Associates:

Alfred John Beaumont.
Isaac William Chubb.
William John Corse.
Rowland Edward Dixon.
John Alfred Edmondson.
Charles Parkin Goode.
R. Holiday.
Frank B. Holt.
Alfred Edward Jackson.
John Ranald Macdonald.
Oscar Albert Pilcher.
Frederick William Wheadon.
Harold Whiteman Woodall.

Students:

Harry Jeffery Bellow.
Henry Herbert Bowlan.
Frederick John Boyle.
Upton P. Brackenridge.
Rowland Francis Browne.
Walter Harvey Chapman.
John Dennis Coales.
Arthur D. Constable.
Edward Dixon.
Owden V. Greeves.
Frank Johnston.
Carl Louis Lichtenberg.
George Fletcher Malden.
Ernest Albert Odhams.
Albert Periam Pyne.
Edward Ray.
Arthur B. Rayner.
Walter Reilly.
Walter Herbert Russell.
Edgar Julius Sander.
Frederick Charles Stephens.
Lennard Carew Trevor-Roper.

ABSTRACTS.

J. J. THOMSON—ON THE VELOCITY OF THE CATHODE RAYS.

(*Philosophical Magazine, Vol.* 38, *No.* 233, *p.* 358.)

The phosphorescence shown by the glass of a discharge tube in the neighbourhood of the cathode has been ascribed by Crookes to molecular bombardment; but Hertz and Lenard have shown that thin films of metal placed between the cathode and the glass do not stop the phosphorence, and hence some physicists doubt Crookes's explanation and support the view that the phosphorescence is due to ethereal waves of very small wave-length, these waves being so strongly absorbed by all substances that it is only extremely thin films which allow their passage.

It follows from the latter view, to which Lenard has been led, that the ether must have a structure either in time or space; for the cathode rays are deflected by a magnet, which, so far as we know, has no effect on ultra-violet light unless this is passing through a refracting substance; thus, if the cathode rays are of the nature of ultra-violet light, either there must be in the ether in a magnetic field a length comparable with the wave-length of cathode rays, or a time comparable with their period of vibration.

To try whether the deflection of the rays is a secondary effect due to the alteration of the distribution of the discharge entering the cathode, and thus of the waves emerging from it, the author shielded the cathode from magnetic forces by a ring of soft iron wire, of length about 1½ inches, and thickness ¼ inch. The result was to prove that the magnet acts on the cathode rays through the whole of their course. Therefore, to prove that the cathode rays are phenomena in the ether, is to prove that the latter has a finite structure.

The author considered that to measure the speed of cathode rays would enable a decision to be come to, as ether waves should have a speed comparable with that of light, while molecules should travel very much more slowly. The method of experiment was as follows:—

The discharge tube was covered with lampblack, except two thin strips in the same straight line, which were left uncovered; these were about 10 centimetres apart, the one nearest being 15 centimetres from the electrode. These strips were chosen so as to phosphoresce about equally when the discharge passed. The light so obtained fell on a rotating mirror 75 centimetres away, driven by a large Gramme machine. The images on the mirror were observed by a telescope; when the mirror was at rest, the images were in the same straight line, and the adjacent ends were brought into coincidence by means of a prism. The displacement of one with regard to the other would form a measure of the speed of the rays. It was

found, however, that both were drawn out into very faint ribbons of light, without definite edges. This is due to the gradual fading of the phosphorescence; and it was impossible to find any substance in which it faded quickly enough. It was found that the beginning of phosphorescence was more definite, and showed a displacement of the images which indicates a time interval between the beginning of the phosphorescence of the two strips. The amount of the displacement varies irregularly, though the mirror is rotating uniformly, probably due to irregularities in the E.M.F. acting on the tube. The speed worked out from these observations is $1 \cdot 9 \times 10^7$ centimetres per second, which is small compared with that of the main discharge, but much greater than that of the mean square of the molecules of gases at 0° C. (about 100 times that of hydrogen molecules). It agrees nearly with that which a negatively charged hydrogen atom would acquire under the influence of the potential fall at the cathode, and the action of the magnetic force in deflecting the rays agrees with the assumption of an electrified body moving at the least with this speed.

L. ZEHNDER—MEASUREMENTS WITH RADIANT ELECTRIC ENERGY.

(*Wiedemann's Annalen, Vol.* 53, *No.* 9, *p.* 162.)

The author describes, in the first instance, how he has determined the index of refraction of his asphalt prism, both by the Meyerstein method of perpendicular incidence and by the method of minimum deflection, and has found but small differences in the two results. They give as the refractive index for rays about 70 cm. in wave-length the value 1·93, against a value of 1·69 found by Hertz for his pitch prism.

It was found necessary to cut off, by means of tin-plate screens, the rays which passed by the prism, and these had a considerable influence on the results, especially when the method of minimum deflection was used. The reason was found to be that two metal screens about 80–100 cm. apart, with their planes perpendicular to the direction of the radiation, can of themselves considerably deflect the rays and give rise to phenomena of diffraction. To avoid this, a diffraction grating was constructed by taking three strips of zinc about 200 cm. long and 33 cm. wide, with their centres 80 cm. apart. This apparatus gave satisfactory measurements of the angle of deflection, δ, from which and the width, $b = 80$ cm., of the grating the value of the wave-length, $\lambda = b \sin \delta$, could be calculated. The value found was 69·4 cm., which was fairly near to the value obtained by the Boltzmann method. From further experiments with various lengths of secondary conductor and the diffraction grating, with a constant length of primary conductor, the author obtained clear evidence of the existence of different wave-lengths among his rays, and of an electrical spectrum. Only the lengths from 67·4 to 73·3 could be measured with his apparatus; but doubtless others both longer and shorter, existed.

Lord RAYLEIGH—ON THE MINIMUM CURRENT AUDIBLE IN THE TELEPHONE.

(*Philosophical Magazine, Vol.* 38, *No.* 232, *p.* 285.)

Noting the extreme discrepancies in the results of other observers, the author considers that what is required is not so much accuracy of measurement as soundness of method; and he thought that electro-motive forces of the required harmonic type would be best secured by revolving a magnet in the proximity of an inductor coil of known construction. In trying to avoid the effects of self-induction the author encountered those of capacity, and the details of the apparatus used for the purpose of avoiding the difficulty are given. When the magnet was driven at full speed the frequency was 307, and at this pitch the value of the minimum current which produced a sound easily audible after a few seconds was $7{\cdot}4 \times 10^{-7}$ amperes, with a resistance of 8,100 ohms; while with a plumbago resistance of 84,000 ohms the current fell to $3{\cdot}8 \times 10^{-7}$ amperes. Experiments at higher pitches were made by means of magnetised tuning-forks vibrating with known amplitudes and having their magnetic axis passing perpendicularly through the centre of the mean plane of the inductor coil. The sensibility of the telephone was found to rise rapidly from $2{,}800 \times 10^{-8}$ amperes at a pitch of 128 to 83×10^{-8} amperes at a pitch of 256, and then to rise more slowly till it attained a maximum at $4{\cdot}4 \times 10^{-8}$ amperes for a frequency of 640. But the only observation above this frequency—at 768—is more or less difficult to obtain; and further observations would be necessary to confirm the fact of a maximum sensibility at about 640 periods per second. This shows a similar limit of sensibility for galvanometers with direct and telephones with alternating currents.

J. B. HENDERSON—ON THE EFFECTS OF MAGNETIC FIELDS ON THE ELECTRIC CONDUCTIVITY OF BISMUTH.

(*Philosophical Magazine, Vol.* 38, *No.* 234, *p.* 488.)

Sir William Thomson first discovered that in iron and nickel magnetisation increases the resistance along the lines of force, and diminishes the resistance in a direction at right angles to them; and Tomlinson first noticed the effect in bismuth. The present investigation was undertaken to determine the relation of the resistance of bismuth wire to the field when high magnetising forces were used, and also to observe the influence, if any, of temperature.

Bismuth is now prepared very pure for instruments for magnetic field testing, and a little spiral, shaped somewhat like a bishop's crozier, was found a convenient form. The two spirals used were 18 and 6 millimetres in diameter, and had resistances of 24 ohms and 9 ohms respectively. The fields were produced by a Ruhmkorff electro-magnet, and for very high values the ring magnet of H. du Bois was employed. The ballistic method was used for measuring the field, the ballistic coil embracing the whole area of the bismuth spiral. To keep the temperature constant a water bath of suitable form was used, and the temperature

was measured by means of a thermo-electric junction. The values for the small spiral at 18° C., being more accurate than those for the larger spiral, are given below. The curves of the percentage increase of resistance for both spirals agree as far as they go—that is, to a field of H = 13,000. The upper four points of the table may have errors of the order two per cent.

Table I.

H.	Resistance, Ohms.	Resistance. / Resistance at H = 0.	% Increase.
0	8·57	1·0	0
5,830	10·54	1·227	22·7
6,310	10·74	1·253	25·3
6,830	11·04	1·290	29·0
7,790	11·47	1·341	34·1
8,880	12·06	1·407	40·7
10,410	12·83	1·496	49·6
12,500	13·97	1·630	63·0
15,710	15·60	1·830	...
20,450	18·57	2·160	...
23,480	20·02	2·333	...
26,820	21·50	2·508	...
27,450	21·76	2·540	...
27,820	22·07	2·568	...
28,370	22·37	2·609	...
29,270	22·65	2·643	...
30,090	23·20	2 704	...
31,270	23·77	2·772	...
32,730	24·40	2·846	...
33,300	24·78	2·893	...
35,800	26·29	3·070	...
36,600	27·03	3·160	...
38,900	28·56	3·334	...

Further series of tests were made with constant fields and varying temperatures, with fields varied from 0 to about 23,000. The tables for zero field H = 7,200, and H = 11,500, and H = 22,700, are here given.

From these curves, when plotted, it will be seen that the inclination of the curve to the horizontal diminishes as the field increases, being at first positive, then zero, then negative; and the curves deviate with increasing fields more and more from straight lines, from which it seems probable that all the curves have

Table II.

H = 0.		H = 7,200.		H = 11,500.		H = 22,700.	
Temperature ° C.	Resistance, Ohms.	Temperature ° C.	Resistance, Ohms.	Temperature ° C.	Resistance, Ohms.	Temperature ° C.	Resistance, Ohms.
0	8·02	...	...	...	...	...	...
6·6	8·22	7·6	11·22	...	...	10·8	19·93
7·0	8·23	28·2	11·27	9·2	13·63	22·6	19·00
12·3	8·40	36·2	11·36	23·8	13·34	26·4	18·72
16·0	8·51	44·2	11·45	33·4	13·25	29·4	18·53
21·7	8·69	51·8	11·55	...	13·15	34·6	18·27
28·8	8·91	58·2	11·64	73·6	13·25	43·4	17·80
36·2	9·16	64·0	11·74	82·6	13·34	52·6	17·40
44·3	9·43	69·2	11·83	...	...	61·6	17·12
54·0	9·76	74·8	11·92	...	...	68·0	16·93
61·9	10·04	79·8	12·02	...	...	77·0	16·74
92·0	11·09	...	...	...	...	81·4	16·64

a minimum of resistance not necessarily on the range of temperature here dealt with; such minima occur at higher and higher temperatures as the field increases.

From the above observations, it is obvious that instruments for the measurement of magnetic fields which utilise this property of bismuth must take accurate account of the temperature. The author suggests winding a spiral of copper or platinum side by side with the bismuth, and determining the temperature from the resistance of this coil.

C. FROMME—EXPERIMENTAL RESEARCHES IN MAGNETISM.

(*Wiedemann's Annalen, Vol.* 53, *No.* 10, *p.* 236.)

The eighth portion of this series of articles contains observations on the self-induction and electrostatic capacity of coils of wire, and their influence on magnetic phenomena. When a piece of iron is magnetised by a coil which produces a magnetising force upon it, and the current is gradually reduced to zero by means of a liquid resistance, the iron retains the largest possible amount of residual magnetism ($P\ M_{max}$). If the current be again made at its greatest value, and broken suddenly, the residual induction is less by an amount we may call $\delta\ P\ M$. If a resistance of a certain value be put in parallel with the current-breaker, the current is not reduced to 0, but to a certain small value, while the path provided for the so-called extra current diminishes the rapidity of the change of induction. If the resistance be gradually increased to infinity, one would anticipate a smaller value for $\delta\ P\ M$ than that obtained by sudden break; but this anticipation is not

always realised. The nature of the resistance used affects the question. If it be free from self-induction or capacity (for instance, a rectilinear liquid resistance, or a Chaperon resistance bobbin in which the direction of winding is reversed at every layer, or a Siemens non-inductively-wound bobbin), then δ P M is smaller than for sudden breaks. But if an ordinary Siemens resistance of two to five thousand ohms be employed, the falling off of the permanent magnetism is much greater than with sudden breaks, except for very small magnetising forces. If a differentially-wound galvanometer coil be employed—first, with the coil coupled to give maximum self-induction, secondly, with only one coil in shunt to the breaker, and lastly, with the coils coupled back to back, the behaviour in the first case differs most, in the second case less, and in the third case little, if at all, from the normal phenomenon.

The form of the magnetised body has influence on the results obtained. The phenomena are most marked when the iron is a bundle of wires insulated from one another, are less noticeable with steel bars, and are quite absent in iron bars. They diminish in strength when the interruptor is quicksilver instead of platinum, or if the bundle of wires be surrounded by a split metal tube, and disappear if the tube be continuous, or when the wires are surrounded by mercury so as to make metallic connection.

In explaining these phenomena, the author considers first the action of the extra current which arises when the circuit is suddenly interrupted. If the parallel circuit has neither self-induction nor capacity, the effect of sudden break is most concealed, as the extra current has freest path. Self-induction reduces the extra current, and therefore its magnetising effect, and therefore increases δ P M. If the shunt contain capacity, it acts as a condenser charged by the extra current The current, therefore, does not run down so quickly, and electrical oscillations may take place whereby δ P M may be increased. The more uniform the running down of the energy, the smaller must be the effect of the self-induction or capacity of the shunt on the residual magnetism.

The author adds a few notes which are of interest in connection with resistances used with alternate currents. "Bifilar"-wound bobbins (for example, Siemens resistance coils) have low capacity up to 1,000 ohms, but at 2,000 the capacity is marked. Chaperon's coils, as far as tested (up to 3,000 ohms), are quite free of capacity, and they have practically no self-induction. This confirms the results of other observers.

S. SKINNER—THE CLARK CELL WHEN PRODUCING A CURRENT.

(*Philosophical Magazine, Vol.* 38, *No.* 232, *p.* 271.)

The experiments were made with the view of ascertaining how far the E.M.F. round the circuit when the cell is producing a current differs from that of the open cell, and how this value changes when the current is maintained. If these quantities can be accurately measured, it follows that a cell might be used for producing currents of no value. In the large Clark cells used the E.M.F. of polarisation, when the cell has a current not greater than 0·01 of an ampere taken

out of it, is very small, and it is this small value that is the subject of the author's experiments. The measurement of the internal resistance was made by coupling two cells back to back and forming one arm of a Wheatstone's bridge, the determination being made by alternating currents to avoid polarisation effects. The difference in the E.M.F. of the two cells was never more than 0·04 per cent. The conclusions arrived at are that the electro-motive force of polarisation varies directly with the current-density of the cell, and that the electro-motive force of polarisation slowly increases when the current is maintained. Small currents of approximately known value can be obtained by the use of large Clark cells of small internal resistance, which may be neglected in comparison with the large external resistance. The largest cell, with a current-density of $\frac{0{\cdot}01 \text{ amp.}}{95 \text{ cm.}^2}$, was not at all disturbed. It follows that an ordinary Clark cell would be uninfluenced if used in series with 7,000 ohms.

W. JAEGER and R. WACHSMUTH—THE WESTON STANDARD CADMIUM CELL.

(*Elektrotechnische Zeitschrift*, 1894, *No.* 37, *p.* 507.)

The authors describe in detail the Weston standard cell, which differs from that of Clark in having the zinc amalgam and zinc sulphate replaced by the corresponding cadmium combinations. The chief advantage of this modification is the lowering of the temperature coefficient, which has a value about 1-23rd of that of the Clark cell. The result of this is that variations of several degrees in the temperature of the cell are of no importance in practical work, while for accurate measurements only a very approximate value need be taken for the temperature of the cell.

A set of experiments was made on the amount of impurity in the component salts which affected the E.M.F. of the cell, and it was found that considerable impurities altered the E.M.F. by an amount of the order only hundred-thousandths of a volt.

The constancy of the cell cannot yet be definitely affirmed; but the oldest, about four months old, show no signs of change. To test the effect of travelling, a cell was sent by post to Frankfort and back, from Berlin, and returned quite uninjured.

The E.M.F. is about 1·025 true volts at 20° C., and the formula for the relation of E.M.F. to temperature is as follows:—

$$E_t' = E_0 - 1{\cdot}25 \times 10^{-5}t - 0{\cdot}065 \times 10^{-5}t^2.$$

G. CHARPY—ON THE TEMPERATURES OF TRANSFORMATION OF IRON AND STEEL.

(*Comptes Rendus*, *Vol.* 119, *No.* 18, *p.* 735.)

The author applied Mr. Osmond's method of studying the dissipation of heat at different temperatures to several samples of steel.

The apparatus consisted of—

1. A furnace, heated by means of an electric current passing through a platinoid wire.
2. A pyrometer of the Le Chatelier type, recording automatically on a sheet of sensitised paper. Curves were taken during heating and cooling.

The following table gives the critical temperatures observed during the heating and cooling of five samples of extra soft steel; these critical points are called by Mr. Osmond A_1, A_2, A_3:—

—	A_1.		A_2.		A_3.	
	Heating.	Cooling.	Heating.	Cooling.	Heating.	Cooling.
Steel with 0·07 of carbon	—	—	740	730	865	840
,, 0·09 ,,	722	664	744	731	903	860
,, 0·07 ,, and 1·15 of nickel ...	710	698	744	732	835	—
Steel with 0·08 of carbon and 0·75 of chromium	—	675	744	741	860	789
Steel with 0·11 of carbon and 0·60 of tungsten	—	630	749	740	923	877

These figures are the mean of several experiments. With all soft metals the point A_1 increases with the amount of carbon, but, of course, in the above experiments it was hardly distinguishable.

The point A_3, generally very apparent, varies not only with the nature of the metal, but also with the rapidity of heating.

The point A_2 is, however, remarkably stable, and is exactly the same either for heating or cooling. Some experiments lately carried out by Mr. Arnold on soft iron also confirmed the stability of the point A_2. Mechanical tests made after tempering showed that the flat part of the curve of elongation disappears when the metal is heated up to 750° or 800° and then tempered. The point A_2 would then correspond to the transformation in the iron produced by hammering when cold, and which characterises the flat part of the elongation curve. This seems to also correspond with the critical temperature of 740°.

These results agree with those of M. Curie, who finds that, from a magnetic point of view, iron behaves normally up to a temperature of 800°—in fact, the transformation of the iron has some effect on the magnetic properties, but its influence would be very weak as compared to that of temperature.

From the above facts the following general conclusions are arrived at:—The point A_1 (690°–700°) corresponds to transformation of the carbon characterised by the Eggertz test—a transformation which considerably increases the lasting properties of the steel; the point A_2 (740°) corresponds to the transformation of the iron characterised by the flat part of the curve of elongation: this

transformation slightly modifies the mechanical and magnetic properties; the point A_3 (860°) corresponds to a second transformation of the iron, which seems to specially influence its magnetic properties.

R. BLONDLOT—ON THE PROPAGATION OF ELECTRO-MAGNETIC WAVES IN ICE, AND ON THE DIELECTRIC POWER OF THAT SUBSTANCE.

(*Comptes Rendus, Vol.* 119, *No.* 15, *p.* 595.)

In a preceding paper (*C. R.*, July, 1892) the statement was made by the author that the length of wave emitted by an electro-magnetic oscillator was the same for all substances; or, in other words, the length of wave depends solely on the dimensions of the oscillator, as in acoustics the length of waves emitted by a pipe depends on the length of that pipe.

The above statement was experimentally confirmed in the case of certain oils used as insulating mediums.

Certain doubts, however, existed in the case of ice, on account of its exceptional properties.

M. Bouty found that ice had a dielectric power of 78, which is incomparably higher than that of other substances.

The following experiments were carried out to ascertain what the law of propagation of waves would be in a medium differing so widely in its properties from that of others.

The researches were carried out during the severe winter of 1892. The method differed slightly from that employed in previous experiments, on account of the solid nature of the dielectric.

The electro-magnetic waves were transmitted along two tinned copper wires, 2·5 mm. diameter, stretched horizontally and parallel to one another, and separated by a distance of 0·8 metre. A resonator of gilt copper was placed in a fixed position between the two wires. The portion of the wires beyond the resonator were led into a wooden trough 4 metres long.

A metallic connection between the wires at a certain point would stop the spark in the resonator, and the distance of this point from the resonator is one-quarter of the length of the wave emitted from the resonator. The position of this point was carefully noted before making experiments with ice.

The portion of the resonator forming a condenser was next surrounded by an envelope of parchmentised paper, filled with distilled water. The water was then allowed to freeze. The length of wave was found, under these conditions, to be much greater than in the first experiment, being about 1·41 times more.

The trough was then filled with water. When this had frozen, the new position of the bridge connection which would stop the spark in the resonator, was noted. It was thus proved that this position is exactly the same as when the dielectric consists entirely of air.

This experiment was repeated four times, with the same results, the capacity of the condenser being altered in each case.

According to Maxwell, the dielectric power is proportional to the square of the index of refraction, and this has been verified in the case of ice. The above results led the author to determine the dielectric coefficient of ice by employing electromagnetic vibrations.

If the wave-length corresponding to any given resonator be called λ and λ_1, working respectively in air and in a substance having a dielectric power K, then $\frac{\lambda_1}{\lambda} = \sqrt{K}$.

As mentioned above, it was found that

$$\frac{\lambda_1}{\lambda} = \frac{141}{100}; \text{ or } K = 2, \text{ in round numbers.}$$

The result was confirmed by a dozen experiments, in which the relative errors would not exceed $\frac{1}{20}$. According to this it would appear that ice does not present exceptional dielectric properties.

A. PÉROT—ON THE DIELECTRIC POWER OF ICE.

(*Comptes Rendus, Vol.* 119, *No.* 15, *p.* 601.)

M. Blondlot pointed out an error made by the author in experiments for determining the dielectric power of ice, the results of which were published in June, 1892. The error consisted in not taking into account the external capacity, since the condenser was entirely surrounded by the dielectric.

The following are the corrected results, confirmed by fresh experiments:—

λ (Air).	λ (Ice).	$\sqrt{K}$.
91	130	1·43
91	133	1·46
136	186	1·37
136	197	1·44
151	215	1·42
149	214	1·44

Mean $\sqrt{K} = 1·43$. $K = 2·04$.

W. DIERMAN—ON THE CALCULATION OF OVERHEAD CONDUCTORS FOR ELECTRIC TRACTION.

(*L'Eclairage Electrique, Vol.* 1, *No.* 9, *p.* 409.)

In designing the conductors for a line of tramways it is first necessary to consider the number of service cars, their capacity and speed, and also the nature of the gradients.

Data must be obtained as to the normal traffic and the maximum possible traffic. A plan and section of the track will also be necessary.

With respect to capacity, it would be well to consider the limits in the case of horse traction. On many old lines the capacity is very small, there being about 24 passengers for closed cars and 26 passengers for open cars. The width would be about 1·5 metres, and their length 5 metres. For a car of this size one horse would be employed. On large lines, cars for 36 passengers, and drawn by two horses, are employed. With such cars as these the service would be from every three to eight minutes. A double track is indispensable when the number of departures exceed ten per hour. On large lines with a frequency of less than eight to ten minutes, cars with 40 to 60 places, including top seats, are used, drawn by two or three horses abreast.

On lines with gentle gradients, the mean speed will not exceed 8 kilometres per hour, with one horse and a single track. On a double track the speed may reach 8·5 kilometres per hour with one horse; and with two or three horses, and not too much traffic, from 9 to 10 kilometres per hour. The maximum speed at any moment will not exceed from 12 to 13 kilometres per hour, except on coming down gradients.

Cars for 26 passengers weigh 1,300 to 1,500 kilogrammes; those for 36 passengers, 1,800 to 2,000 kilogrammes; and those for 40 to 60 passengers, 3,000 to 4,000 kilogrammes.

On a line on which the traffic is fairly limited, electric cars with two axles would be employed, with as large a capacity as permissible with this type of car. The capacity which realises the minimum dead weight per seat is that which corresponds to a car 4·9 metres long, with 1·2-metre platforms. Such a car would weigh 5 tons, would be fitted with two motors, and would carry 36 passengers. The dead weight per passenger is $\frac{500}{36} = 139$ kilogrammes. The dead weight of a car with 26 seats, fitted with 20 10-H.P. motors, would be 4,225 kilogrammes, or $\frac{4,225}{26} = 163$ kilogrammes per passenger. These figures are on the assumption that the cars are fully loaded, which is, of course, not always the case.

It is advisable to limit the capacity of the cars to the minimum possible amount, and to carefully consider the number of passenger-kilometres before having recourse to a long two-bogie car with more than 36 seats. This type of car is more costly per seat than the two-axle car, but, on the other hand, the staff expenses are reduced. The weight per seat is greater. In the United States, large cars are only employed in large towns having a regular and heavy traffic, and in which the cost of labour is high.

The electrical power per seat is about the same in the two cases, as shown from tests made by Mr. Dickinson on two cars on the South Staffordshire line of tramways. Two-bogie car: Capacity, 50 passengers; mean speed, 11·7 kilometres per hour; mean power, 13 H.P. Two axle car: Capacity, 40 passengers; mean speed, 11·65 kilometres per hour; mean power, 10 H.P.

Calculation of Overhead Conductors.

The power required for each car is dependent on its position on the plan of the line, and also, as seen, on the section of the track; taking also into consideration the speed and frequency of service.

The author then considers the questions under several headings.

(a.) Crossings and Shunts.

If L is the total length of line, v the mean speed, t the frequency of service, l_v the distance between two consecutive cars, l_c the distance between two crossings, n the number of cars in motion (there may also be a car at rest at each terminus), $n - 1$ the number of crossings, then

$$L_c = \tfrac{1}{2}\, v \frac{t}{60};$$

$$L_v = v \frac{t}{60}.$$

If t_a be the time of stoppage of each car at each terminus, and if a represent the distance between the last crossing and the terminus, and v_m the new corresponding speed, then

$$v_m = t \frac{60\ L}{[1 + \tfrac{1}{2}(n - 2)] - t_a}.$$

And if
$L = 12{\cdot}370$ kilometres,
$t = 3{\cdot}5$ minutes,
$v =$ less than 14 kilometres per hour,

then $n = 32$ cars.

If $t_a = 2$ minutes, $v_m = \dfrac{742}{3{\cdot}5 \times 16 - 2} = 13{\cdot}8$ kilometres per hour.

This mean speed is of use in determining the work of traction.

(b.) Maximum Speed.

The motors must be designed for a maximum speed, greater than the average speed by an amount which is easily determined.

If
N is the number of stoppages per kilometre run,
Nv the number of stoppages per hour,
t_r the time of slowing up preceding each stoppage,
t_d the time for starting after each stoppage,
t_a the time of stoppage,
d_r the distance covered during slowing up,
d_d the distance covered during starting,
$Nv_m\, t_a$ the time due to stoppage per hour

(time expressed in minutes, and distances in kilometres), the time of running at full speed per hour will be,

$$60 - Nv_m(t_r + t_d + t_a);$$

and the distance covered in that time will be,

$$v_m - Nv\ (d_r + d_d).$$

$$V_{max} = \frac{v_m - N\,(d_r + d_d)\, v_m}{60 - Nv_m(t_r + t_d + t_a)}.$$

And if
$t_r = 5''$, $d_r = 10$ m.,
$t_d = 10''$, $d_d = 30$ m.,
$t_a = 15''$, $v_m = 13{\cdot}8$,
$N = 4$,

then $V_{max} = 21{\cdot}5$ kilometres per hour.

It is at this speed that the electro-motors should develop the maximum power.

(c.) Work of Traction.

It is generally admitted that the coefficient of traction on a tramway is about 15 kilogrammes per ton. It may, however, be as low as 12 kilogrammes on a well-laid road, and as high as 18 to 20 kilogrammes. The condition of the roads is a very variable factor.

All tracks employed for horse traction laid on wooden or metallic girders would require an expenditure of 8,000 to 10,000 francs per kilometre to render them serviceable for electric traction, with cars weighing from 3½ to 4 tons per axle. Under these conditions, and with a clean road, the coefficient of traction would be about 12 kilogrammes per ton.

With a bad road and grooved rails the coefficient may easily be doubled.

The weight of a loaded car is about 7 tons with one 20-H.P. motor and 7¾ tons with two 15-H.P. motors. The single motor is only suitable when the gradients do not exceed 4 per cent. on a good road, unless the two axles be coupled together in some suitable manner.

The author then gives for an imaginary line the diagram of traction efforts for both tracks. A curve is also given showing the efficiency of the motors at different loads. From these it was found that the total amount of electrical work expended amounted to 13,150 watt-hours for a return journey of 2·464 kilometres, which, however, does not include the start. It is calculated that 50 watt-hours are required every time a car is started. If there are about 98·5 starts for the go and return journey, the amount of energy expended will amount to 4,928 watt-hours. The total will then be 18,078 watt-hours.

From the diagrams mentioned above the author ascertains the losses in the conductors due to the motion of the car, which is a function of the resistance of the line and of the strength of the current.

The combined sections of the conductor and distributor must be made sufficiently large to reasonably limit the losses. When the distributor does not also act as feeder, and as long as its length does not exceed 30 kilometres, the total section of trolley wire and distributor may be taken at about 8 square millimetres per car per kilometre on level tracks; the sections over the gradients must be increased according to the power taken by the cars. The minimum section would be about 50 mm^2.

The author works out the losses in the case of the above imaginary track with double trolley wires. Loss in trolley wire and distributor = 102 watt-hours for the go and return journey of a car absorbing 18,250 watt-hours, which make a loss of 0·56 per cent. The loss will remain constant with four cars working on the four sections into which the line is divided. If, however, there be the full number of 32 cars, the loss will amount to 4·48 per cent., and the efficiency of the line is 95·7 per cent. It is next supposed that, instead of a double trolley, the rails are used as a return. These will consist of 12,320 kilogrammes for the double track in about 1,232 lengths. The rails weigh 30 kilogrammes per metre run, or 120 kilogrammes per metre of the double track. The net section will amount to 154 sq. cm. The combined resistance of rails and joints will amount to 0·0201 ohm. The loss in the rails amounts to 9·78 watt-hours when the car absorbs 18,250 watt-hours, or about 0·05 per cent.

The loss in the distributors and trolley wires is			4·48	per cent.
,,	,,	,, rails is	0·43	,,
	Total	... ...	4·91	,,

The author points out the advantage of employing a graphical method for calculating tramway circuits. It is the usual practice to assume a certain maximum loss, thus leading to the use of too much copper, or in many cases a mean loss due to a mean current is falsely assumed.

ANON.—THE PACIFIC CABLE.

(Journal Télégraphique, Vol. 18, No. 11, p. 317.)

With this article is supplied a map showing the alternative positions suggested for the Pacific Cable. At the annual Australasian Postal-Telegraph Conference, held at Wellington, New Zealand, in March last year, Mr. Sandford Fleming gave his views in favour of this enterprise, and pointed out the several paths which might be adopted for laying the cable. His propositions consisted of eight alternative paths, all starting from Vancouver—

No. 1. Passing by Fanning Island, bifurcated at Fiji or at Norfolk Island, one route terminating in Queensland or New South Wales, the other in the North of New Zealand.

No. 2. Passes through Necker Island (Sandwich Islands), and thence to Fiji; bifurcates at this point or at Norfolk Island, the two routes ending respectively at the same points in Australia and New Zealand.

No. 3. Also passed through Necker Island, and then went on to the Gilbert Islands; it then bifurcated at this point, one route terminating at Bowen (Queensland), and the other in the North of New Zealand.

No. 4. This route went direct from Vancouver to Bowen through Necker Island.

No. 4A. Followed the same route as the last one, but extended by land line from Bowen to Sydney (New South Wales), and by cable from Sydney to the North of New Zealand.

In the resolution proposed by Mr. J. Ward at this conference, the following route was suggested:—From the coast of Queensland (Brisbane) or from the North of New Zealand (Alupara Bay) to Vancouver, passing by the Fiji Islands (Suava Island), the Samoa (Apia, in the Isle of Cpulu), Fanning Island, and the Sandwich Islands.

Mr. Alexander Siemens proposes laying the cable between Alupara Bay, on the New Zealand coast, and Vancouver, by passing through the Fiji, Phœnix, and Necker Islands, following the same line as one of the propositions made at the Wellington Conference.

New soundings would be unnecessary, as the route proposed at the conference has no greater depth than 3,000 fathoms, and it is now possible to lay a cable in depths of 3,000 fathoms without exactly knowing the configuration of the bottom.

HENRI MOISSAN—ON THE VAPORISATION OF CARBON.

(*Comptes Rendus*, *Vol.* 119, *No.* 19, *p.* 776.)

Until now the formation of carbon vapour was studied either by spectrum analysis, or by Berthelot's synthesis of acetelyne.

It is, however, possible to demonstrate the vaporisation of carbon outside the arc in the following manner:—If a carbon tube having an internal diameter of about 1 centimetre be placed in a lime furnace heated by a powerful arc (2,000 amperes, 80 volts), the interior of the tube will be found to cover quickly with a black deposit, produced by the condensation of carbon vapour.

It is also possible to show the presence of this vapour by placing some crystallised silicium in the centre of the tube, which on heating will be found to melt and boil, and then to combine with the carbon vapour to form transparent crystals of silicate of carbon.

After having thus proved that carbon vapour can be produced without the presence of the electric arc, experiments were next made to ascertain in what manner the vapour was produced, and whether the carbon passed into the liquid state before vaporising.

A small crucible of pure carbon, with a deep and well-fitted lid, was placed on a disc of carbon supported on compressed magnesium. It was heated with an arc of 1,200 amperes and 80 volts. The temperature was high enough to volatilise several hundred grammes of lime and magnesia. When cold, the crucible was found converted into graphite, but the lid was not cemented to the crucible in any way. The same result was obtained with wood carbon and retort carbon.

This would not, however, be the case if the carbon contains metallic impurities —silicon or boric acid. In the latter case crystals of Bo^6 C would be formed. Very small quantities of metallic impurities may form carbides in a fused or crystalline state.

In carrying out such experiments it is essential that absolutely pure carbon should be used. In order to study the varieties of carbon produced by condensation the vapour was collected in three different ways—

1. By distillation. The carbon vapour condensed in the carbon tube, as described above, yielded a black deposit formed entirely of graphite.
2. By condensation on a cold body. When a copper tube cooled by means of a current of cold water was placed in the electric furnace, its surface was covered with a black deposit, which was treated with hydrochloric acid to eliminate all traces of lime. This deposit was found to contain silicon and other impurities, but was found to consist chiefly of microscopic crystals having all the characteristics of graphite.
3. By condensation on a heated surface. When the electric arc is formed in a furnace containing lime (for the purpose of preventing the formation of carbonic vapour, which would convert the carbon vapour into carbonic oxide), one obtains, especially at the positive pole, carbon mushrooms, produced by vaporisation in the arc itself. This carbon, of which the surface is more or less rounded, has no appearance of fusion. Its density is 2·1. By analysis it was found to contain 99·6 to 99·9 per

cent. of carbon, and but an inappreciable amount of ash. This pure carbon produced by distillation has all the characteristics of graphite, and can only be burned in oxygen at a fairly high temperature.

The author found that throughout these experiments the carbon produced by condensation existed always in the form of graphite.

Investigations were next made as to the nature of the deposit produced on the bulbs of incandescent lamps. The deposit was collected in a glass containing water. Microscopic examination revealed very small crystals of silicate of carbon, also some small black masses which presented no signs of crystallisation.

It was also noticed that a chestnut-coloured film floated at the surface of the liquid.

It was proved that the deposit in incandescent lamps consists almost entirely of graphite.

If the ends of a broken incandescent lamp filament be examined microscopically they will not present any signs of fusion, but are covered with small crystals of graphite.

In all these experiments it was conclusively proved that in vacuo, as at ordinary pressures, carbon passes from the solid state to the gaseous state without going through an intermediate liquid state, and in this respect can be compared with arsenic.

When gaseous carbon solidifies it always produces graphite.

The author considers that carbon could be liquefied at very high pressures. Under these conditions the density would increase and the diamond would be formed; and, indeed, small diamonds were obtained having the form of an elongated drop.

Carbon under pressure can, then, assume the liquid condition and solidify, like water, to either form a crystalline mass or an amorphous rounded mass.

L. CAILLETET and E. COLLARDEAU—RESEARCHES ON THE OCCLUSION OF ELECTROLYTIC GASES BY POROUS BODIES, AND ESPECIALLY BY METALS OF THE PLATINUM GROUP: APPLICATION TO A GAS BATTERY; THE EFFECT OF PRESSURE ON ELECTRIC ACCUMULATORS.

(*Comptes Rendus*, *Vol.* 119, *No.* 20, *p.* 830.)

It is known that in the electrolysis of water by platinum electrodes it takes an appreciable time for the hydrogen and oxygen to appear after the current has been established; also when the current is stopped there exists a difference of potential in the cell, such as will produce a current in the opposite direction when the circuit of the voltameter is closed on itself, this phenomenon being due to the recombination of the two gases occluded by the electrodes. The authors attempted to construct a gas battery having a greater capacity than, and none of the disadvantages of, the Grove battery.

Electrodes were first used consisting of about 6 grammes of spongy platinum tied up in small silk bags, the current being led in and out by platinum wire.

These electrodes were immersed in a 10 per cent. solution of sulphuric acid. The capacity obtained was much greater than with an equal weight of platinum in the ordinary metallic condition. The next experiment was for the purpose of ascertaining whether this phenomenon of occlusion would be altered under very great pressures.

The voltameter was placed in a steel chamber, and a pressure of 600 atmospheres obtained by means of a hydraulic pump.

It was ascertained that the capacity was greatly augmented by the application of this pressure. Curves are given in which the discharge current is plotted as a function of the time.

At atmospheric pressure the time of discharge is only about 10 seconds. The initial pressure is about 1·8 volts, and falls rapidly to zero.

When operating at high pressures the discharge curve is formed of three distinct steps—

1. A steep fall followed by a slight increase.
2. A period of constant current, which increases slightly with the pressure. During the period the E.M.F. is approximately 1 volt.
3. A further drop, but less rapid than the first.

An accumulator containing 1 kilogramme of spongy platinum at 580 atmospheres could yield 50 ampere-hours, whereas a lead accumulator with the same weight of active material would only yield from 10 to 20 ampere-hours. The discharge current in the former case can easily attain 100 amperes per kilogramme.

It was noticed in these experiments that when equal weights of material were used for each electrode hydrogen was given off much before oxygen.

It was found that for the best output there should be three parts of negative electrode to one part of positive electrode.

The efficiency of the cell was found to be 95 to 98 per cent. when the charge was not pushed to the last limits and when discharge takes place immediately.

These experiments were also carried out on other metals belonging to the platinum group.

Iridium yielded results almost analogous to platinum.

Ruthenium is slightly attacked at the positive pole by the acid, and is coloured to a dark brown. The gases are occluded by this metal, but the E.M.F. never reaches a constant period, and diminishes continuously from 1·6 volts to zero at a pressure of either 1,000 atmospheres or at atmospheric temperature.

Palladium yielded very interesting results. An accumulator made up of two plates of palladium was found to have but a very feeble capacity even under high pressures, this being attributed to the almost immediate absorption of gases by the positive pole.

On employing the metal in the spongy condition the results were quite different, the capacity being greater than with any other metal of the platinum group.

With equal pressures and weights of active material the capacity with palladium is from three to four times greater than with platinum. At a pressure of 600 atmospheres the capacity is 76 ampere-hours per kilogramme of spongy palladium. The capacity obtainable with gold (prepared from the chloride) is also

dependent on pressure, and is not so great as with either palladium or platinum, and the forms of the discharge curves are also somewhat different. In the case of silver, tin, nickel, and cobalt, tested under the same conditions, there is a chemical action on the metal at the positive pole, which is also the case with carbon under its different forms.

Although these metals are capable of storing a certain amount of chemical energy, it is found that any increase of atmospheric pressure is incapable of increasing their capacity. These results are no doubt attributable to certain chemical combinations such as take place in the lead accumulator.

In conclusion, it appears that amongst the different substances tested, the noble metals, which are not chemically attacked by the electrolyte or products of combustion, are alone capable of forming gas batteries of which the capacity increases with atmospheric pressure, and with certain of these metals the capacity is even greater than with the lead accumulator.

M. Berthelot points out, with reference to the above communication, that platinum and palladium and analogous metals enter into combination with free oxygen and hydrogen when cold. Platinum notably forms two successive hydrates, of which one is stable up to 200°, and the other is unstable when cold; and it is especially the latter which is influenced by pressure. This would account for the current obtained from two platinum electrodes immersed in acid and without employing any auxiliary E.M.F.

M. CH. MARÉCHAL—AN ELECTRO-CHEMICAL ACTINOMETER.

(*Bulletin de la Société Internationale des Electriciens, Vol.* 11, *No.* 112, *p.* 437.)

The action produced by light on sheets of metal immersed in different solutions was first noted by Becquerel, who, after concentrating his researches on haloid of silver compounds, produced the sub-chloride of silver actinometer.

A few years ago MM. Gouy and Rigollot found that a sheet of oxidised copper which is immersed in a solution of some metallic chloride, bromide, or iodide becomes very sensitive, even to weak light, and could be employed as an actinometer. The author, in conjunction with M. Rigollot, has worked on the same lines, and devised the actinometer described below; the experimental results obtained only being published in part.

The actinometer in its elementary form consists of two sheets of copper immersed in a solution of salt and water, one of which is oxidised, and is exposed to the action of light; the other is not oxidised, and is protected against the action of light either by surrounding it with parchment or paper, or by placing it directly behind the first sheet at a distance of about 0·001 metre.

The plate of copper is oxidised by placing it on a second piece of copper and then heating it over a Bunsen burner until it has assumed an even orange-red tint. If the copper is oxidised further the sensitiveness is decreased. The side of the oxidised plate which is not exposed to light is insulated either with shellac or paraffin. The effect produced by light is instantaneous, and ceases immediately. On open circuit, diffused daylight produces an E.M.F. of several 1,000ths of a

volt, and with direct sunlight a little more than 1-10th of a volt. With a Thomson reflecting galvanometer it is possible to detect the effect due to a candle placed several metres away. The solution was water containing 1-1,000th part of iodide of potassium.

A series of experiments were carried on to find the effect produced on the E.M.F. by the different colours of the spectrum. For this purpose a Thomson galvanometer of 12,000 ohms was used.

The results were plotted as curves, of which the abscissæ represent length of wave and the ordinates the number of divisions on the galvanometer scale. The experiments were made on bromide, chloride, and iodide of sodium.

With sodium chloride the sensitiveness of the actinometer increases gradually and almost regularly from the red to the point of maximum sensitiveness with the violet-blue. The sensitiveness rapidly diminishes with the violet waves. The apparatus is most insensitive with short wave-lengths.

With iodide of sodium the sensitiveness of the actinometer increases gradually up to the blue-green waves, and then decreases gradually. Experiments were next made to verify Egorofft's theory that the intensity of the current is proportional to the square of the distance of the radiant source to the radiated surface. When using the Drummond light it was found that the E.M.F. of the actinometer varies as the inverse square of the distance so long as the light acting on the actinometer has a weak intensity. The E.M.F. produced by the action of light can be greatly increased by coating the oxidised plate with such colouring matters as eosine, erythrosine, safranine, crystal green, malachite green, soluble blue, &c.

The plate, after having been treated in this way, and soaked in a dilute solution of iodide of sodium or potassium, will retain its sensitiveness for long periods. The amount of colouring matter left on the plate is very small.

Good results are also obtained by dipping the plate, after being oxidised, in a weak solution of white gelatine. After drying, the side not exposed to light is coated with paraffin. Plates thus prepared can be kept for long periods without loss of sensitiveness. In studying the effect of lights of different colours on the dyed oxidised plate, it was found that the position of the absorption band was of great importance. This action is analogous to that of orthochromatic photographic plates which are rendered specially sensitive to any particular coloured light by staining them with suitable dyes.

The following table gives the results of an experiment in which crystal green was used:—

	λ						
	0ᵘ·684	0ᵘ·650	0ᵘ·600	0ᵘ·550	0ᵘ·500	0ᵘ·450	0ᵘ·410
	Div.	Div.	Div.	Div.	Div.	Div.	Div.
Non-sensitised plate ...	16	18	80	190	208	200	88
Same plate sensitised ...	140	760	600	408	380	288	168

Plates sensitised with malachite green are about eight times more sensitive than those which are merely oxidised, the maximum electrical effect also extending as far as the red waves.

It was experimentally proved that none of the above effects were due to thermo-electric action, or either to the chemical or calorific power of light, but

solely to its actinic power. When the two plates of copper are dipped into the solution, a certain initial E.M.F. is produced, which decreases after a few minutes to reach a steady condition. This permanent E.M.F. is due to the partial depolarisation produced by the oxide on the illuminated plate.

If two copper plates are placed in the solution, both being oxidised in identically the same manner; and if one is illuminated and the other not, it is found that the direction of the current outside the cell is always from the illuminated plate to the one which is not.

If the actinometer be connected up to a Daniell cell for periods varying from 15 seconds to two minutes, and if the effect produced by light be tested after each polarisation, it will be observed that—

1. The E.M.F. becomes greater after each stage.
2. The effect produced by light is independent of the permanent E.M.F. of the cell.

If two sheets of copper be heated to a red heat and well cleaned with emery paper, but not oxidised or paraffined, and then immersed in a solution of potassium iodide of 1-1,000th, it is found that the permanent E.M.F. of the cell is insignificant. If, however, the effect produced by light is tested after polarising with a Daniell cell for 5 seconds, 1, 4, and 10 minutes, it will be found—

1. That light has no effect on one plate.
2. The effect on the other plate increases with the time of polarisation.

If a similar experiment be made with plates paraffined on the sides facing one another, identically similar results will be obtained. At first sight it would appear that light falling on the oxidised plate produces an allotropic change in the oxygen, and of which the oxidising and electro-negative properties increase proportionally with the luminous intensity which produces them.

A hypothesis offered by the author is based on Vogel's experiments on spirits of turpentine. Light in the presence of ozonised oxygen becomes a depolarising agent, which causes the sense of the two opposing E.M.F.'s to be the same. But, as remarked above, the E.M.F. increases proportionally with the luminous intensity, and this often exceeds the initial E.M.F. Light then produces in the actinometer other phenomena than that of polarisation.

In order to ascertain the exact nature of the reactions taking place in the actinometer, M. Rigollot made use of very concentrated solutions in order to obtain the products formed by the passage of the electric current more rapidly and in greater quantities.

Two actinometers were made up, each having about 6 sq. cm. active surface. One of these was placed in the dark, and the other fully exposed to light. In each was found a greenish precipitate deposited at the bottom of the cell. At the end of five days the liquid of each actinometer was agitated. It was found that the coloration of the one placed in the dark was very weak with respect to the one exposed to light.

A number of experiments proved that this precipitate was due to an electro-chemical effect; and, moreover, it was found above that the E.M.F. increased with the light intensity.

On analysing the precipitate, it was found to consist of cupric chloride,

bromide, or iodide (according to the salt employed). The compound was probably an oxy-chloride, oxy-bromide, or oxy-iodide of copper.

If sodium iodide be used the reaction is represented by the following equation:—

$$4\,Cu\,O + 2\,Na\,I + H^2\,O = 2\,Na\,H\,O + Cu\,I^2\,Cu\,O.$$

With reference to the applications of the actinometer, it is proposed to use it for transforming optical telegraphic signals into electrical signals to be recorded on any ordinary telegraphic instruments.

The results at 8 to 10 kilometres with sunlight and during favourable weather were somewhat promising; but no effect could be produced during the day with artificial light, and not even with the arc light.

A further experiment was made by coupling up one of the poles of the actinometer to one end of an artificial line of 4,500 ohms resistance and of 142 microfarads capacity; the other pole of the actinometer being connected to earth. In order to get rid of earth currents, the line was connected at its other end through a condenser of 27 microfarads capacity and a receiving instrument to earth.

This arrangement was analogous to a submarine cable. When light was thrown on the actinometer the receiver immediately responded.

In the case of submarine telegraphy, the actinometer, when suitably arranged, becomes essentially a luminous relay of great rapidity, allowing light signals emitted from the most delicate instruments to be recorded on telegraphic instruments such as the Morse, Hughes, or syphon recorder. The actinometer will also be of great value in the laboratory in researches on light, electricity, radiophony, photometry, &c.

It has also been of service in studying the brilliancy of the sky under varying conditions.

The galvanometric records obtained by means of an actinometer exposed at Lyons during a very clear day, showed that the maximum light intensity was at about 12.30 p.m. Experiments made at Paris under exactly the same conditions showed that the maximum intensity was at about 1 p.m.; and these results were compared with the well-known phenomena of daily variations in terrestrial magnetism, principally those of declinations of which the maximum effect in the east takes place at Paris at 1 p.m.

During experiments made with the actinometer illuminated by the electric arc it was noticed that the E.M.F. was subject to rapid variations, produced by changes in the intensity and colour of the arc. From these results it is possible to imagine that any variations in the solar radiations might have an electro-magnetic reaction on the earth. In fact, it was found last year that great magnetic perturbances recorded in certain observatories were contemporaneous with the existence of sun spots.

The author concludes by maintaining the importance of a careful scientific study of actinic phenomena, especially by those interested in electro-magnetic and meteorological phenomena; and he also considers that registering electro-chemical actinometers should be used in observatories in conjunction with magnetic variation instruments.

D. HURMUZESCU—ON THE ELECTRO-MOTIVE FORCE OF MAGNETISATION.

(*Comptes Rendus, Vol.* 119, *No.* 24, *p.* 1006.)

An electro-motive force is set up when a difference in magnetisation is produced between two electrodes of the same magnetic material, and dipping in a liquid by which they are chemically attacked.

This phenomenon has been noticed by many experimenters, but all do not agree as to the sense of the electro-motive force.

The author's investigations were carried out under the following conditions:—The electrodes consisted of wires 1 mm. diameter making contact with the liquid at well-polished surfaces, and lying in a certain known position with respect to the magnetic field. The electrodes were fixed in the two vertical branches of a glass tube, one of them being placed between the poles of a powerful electro-magnet, the other being outside the field. The tube contained a solution of acetic acid or of oxalic acid in distilled water, which was quite free of air. The electro-motive forces were measured with a capillary electrometer used as a zero instrument. This has great advantages over the ordinary galvanometer for such experiments as the above, as it allows of the use of liquids containing very little acid, thus making the chemical action slower, minimising polarisation, and eliminating variations in resistance.

The second measurement was that of H, for which a ballistic galvanometer was employed. The electro-motive force of magnetisation may be expressed by the following equation:—

$$E = \frac{l}{2\delta} \cdot \frac{J^2}{k} - \frac{l'}{2\delta'} \frac{J'^2}{k'} \quad \dots \quad \dots \quad \dots \quad (1)$$

l, δ, l', δ' being values of the electro-chemical equivalents and specific weights of the materials employed; k and k' are mean values of a certain function of the magnetisation in the iron and in the liquid.

With all the metals used it was found that the electro-motive force was independent of the sense of magnetisation.

The following are the results of numbers of experiments made on different samples of iron, nickel, and bismuth:—

With Iron.—It was always found that the magnetised iron was positive with reference to the non-magnetised iron. The values of the electro-motive force for different values of the magnetic field are given in the following table:—

E.	H.	E.	H.
5	397	155	3,068
22	739	165	3,321
44	1,263	172	3,682
79	1,781	176	3,718
87	2,038	198	4,729
106	2,268	210	5,436
113	2,452	222	6,240
124	2,512	229	7,040

E being in 10^{-4} volts, H in C.G.S. units.

The values of E and H if plotted in a curve show a point of inflection for H = 2,400. The curve has the following law:—

$$E = \frac{l}{2\,\delta}\,\frac{I^2}{k} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \qquad (1')$$

With Nickel.—The curve of electro-motive forces as a function of the magnetic field presents the same shape and sense as in the case of iron, but without showing the point of inflection, and the values of the ordinates for mean values of the magnetic field do not exceed 1-1,000th of a volt.

With Bismuth.—1. The magnetised electrode is negative with respect to the non-magnetised electrode, and the highest value of the electro-motive force obtained with the strongest magnetic fields does not exceed 1-10,000th of a volt.

2. If the electrode to be magnetised is placed in the direction of the magnetic field, and if contact be made with the liquid at its extremity, then on exciting the electro-magnet there will be set up an electro-motive force between the two electrodes. The one in the magnetic field becomes negative with respect to that outside the field. This electro-motive force, although smaller than in the first case, increases with the richness of the salt contained in solution. With a liquid almost free of iron salt the electro-motive force is zero, and even changes in sign under certain conditions.

These anomalies might be explained by assuming $\frac{l}{\delta} = \frac{l'}{\delta'}$, in formula (1):

then
$$E = \frac{l}{2\,\delta}\left(\frac{I^2}{k} - \frac{I'^2}{k}\right);$$

and also that, owing to the demagnetising force, the magnetic intensity at the extremity of the electrode placed in the magnetic field may become very small, whereas in the liquid itself this intensity may be very great, so that

$$\frac{I'^2}{k'} > \frac{I^2}{k};$$

and all the more so that k is greater than k'.

If in all cases one considers the magnetisation of the salt of iron produced by chemical reaction, the magnetised iron becomes positive with respect to the non-magnetised iron; the same for nickel, and the opposite for bismuth.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the months of DECEMBER, 1894, and JANUARY, 1895.

S. denotes a series of articles. **I.** denotes fully illustrated.

ELECTRIC LIGHTING AND POWER.

P. Boucherot—Distribution of Power and Light by Polyphase Currents at the Workshops of the Weyher & Richemond Co.—*Bull. Soc. Int.*, vol. 11, No. 113, p. 482 (I.).

C. E. Guillaume—On Elastic Couplings.—*Ibid.*, p. 503.

A. Monmerqué—Electric Mains and Conduits in Paris.—*Ecl. El.*, vol. 1, No. 14, p. 626 (I.).

E. Rathenau—Accumulator Station for the Electric Lighting of the Thiergarten District in Berlin.—*E. T. Z.*, 1894, No. 49, p. 662 (I.).

Anon.—Electric Lighting and Tramway Station of the Town of Zwickau.—*E. T. Z.*, 1894, No. 50, p. 686 (I.).

Anon.—Workshop Notes.—*Ibid.*, p. 691.

G. Richard—The Mechanical Applications of Electricity.—*Ecl. El.*, vol. 1, No. 1, p. 3 (S. I.).

Tchikoleff, Klasson, and Turin—On the Illuminating Power of Electric Light Projectors.—*Ecl. El.*, vol. 1, No. 1, p. 8, No. 2, p. 49 (I.).

Anon.—The Cardiff Central Electric Light Station.—*Ecl. El.*, vol. 1, No. 1, p. 24 (I.).

A. Monmerqué—Electric Light Conduits in Paris.—*Ecl. El.*, vol. 1, No. 3, p. 97 (I.).

Anon.—Electric Light Projectors and their Employment in War.—*Ecl. El.*, vol. 1, No. 4, p. 149 (I.).

G. Granquist—Researches on the Electric Arc.—*Beibl.*, vol. 19, No. 1, p. 97.

A. Weinhold—The Electricity Works of the Town of Chemnitz.—*E. T. Z.*, 1895, No. 1, p. 2 (I.).

M. Meyer—The Development of Municipal Electricity Supply Stations.—*E. T. Z.*, 1895, No. 2, p. 26.

Anon.—The Report of the Cologne Municipal Electricity Works.—*Ibid.*, p. 32.

Anon.—Workshop Notes.—*E. T. Z.*, 1895, No. 4, p. 58 (S. I.).

ELECTRIC LIGHTING AND POWER.

— **Blondél**—On Elastic Coupling of Machines.—*Bull. Soc. Int.*, vol. 12, No. 114, p. 18.

E. **Desroziers**—Regulation of Machines by Elastic Couplings.—*Bull. Soc. Int.*, vol. 12, No. 114, p. 30.

DYNAMO AND MOTOR DESIGN.

E. **Kolben**—Influence of the Form of Curve of Alternators on the Working of Motors.—*E. T. Z.*, 1894, No. 51, p. 698 (I.).

— **Pietzker**—New Construction of Dynamos without Iron.—*Ibid.*, p. 704.

R. **Malagoli**—The Alternating Rotary Field, and its Utilisation.—*Ecl. El.*, vol 1, No. 1, p. 1 (I.).

L. **Legrand**—Theory and Design of Asynchronous Motors with Rotary Magnetic Fields.—*Ibid.*, p. 19, No. 2, p. 56, No. 3, p. 99 (I.).

C. P. **Steinmetz**—The Theory of the Synchronous Motor.—*E. T. Z.*, 1895, No. 2, p. 26, No. 3, p. 38.

H. **Cahen**—On the Calculation and Design of Polyphase Motors.—*E. T. Z.*, 1895, No. 4, p. 52, No. 5, p. 64.

ELECTRIC TRACTION.

— **Vuilleumier**—The Electric Tramway from the Pont Lafayette to the Lyons Exhibition—Claret-Vuilleumier System.—*Bull. Soc. Int.*, vol. 11., No. 113, p. 509 (I.).

J. **Reyval**—Electric Traction on Steep Grades.—*Ecl. El.*, vol. 1, No. 14, p. 641, No. 15, p. 680.

Anon.—The South Staffordshire Tramway.—*E. T. Z.*, 1894, No. 49, p. 665.

Anon.—Electric Railways in Berlin.—*Ibid.*, p. 670, No. 50, p. 693.

Anon.—Opening of the Bochum-Herne Electric Tramway.—*Ibid.*, p. 670.

Anon.—Electric Lighting and Tramway Station of the Town of Zwickau.—*E. T. Z.*, 1894, No. 50. p. 686 (I.).

Anon.—The Budapest Underground Electric Railway.—*Ibid.*, p. 694.

Anon.—Electrical Tramway in Danzig.—*E. T. Z.*, 1894, No. 51, p. 702.

Anon.—Electric Warming of Tram-Cars.—*Ibid.*, p. 703.

Anon.—Electric Tramways in the United States and Canada.—*E. T. Z.*, 1894, No. 52, p. 713.

Anon.—Electric Tramways in Munich.—*Ibid.*, p. 718.

Anon.—Experiments on Central Stations for Electric Tramways.—*Ecl. El.*, vol. 1, No. 2, p. 70.

Anon.—Curves on Trolley Lines.—*Ibid.*, p. 76.

T. **Tomasina**—The Electric Tramways in Geneva.—*Ecl. El.*, vol. 1, No. 3, p. 123.

J. **Reyval**—Electric Traction with Conductors in Conduit in America.—*Ecl. El.*, vol. 1, No. 4, p. 145.

ANON.—Signals for Single-Track Tramway Lines.—*Ibid.*, p. 174 (I.).

L. KOHLFÜRST—Hattemer's Uni-directional Approach Signal.—*E. T. Z.*, 1895, No. 1, p. 10 (I.).

ANON.—Experiments on Accumulator Traction in Vienna.—*E. T. Z.*, 1895, No. 2, p. 35.

ANON.—Wear of the Trolley Wire in Electric Tramways.—*Ibid.*, p. 35.

C. P. FELDMANN—Electric Tramways with Accumulator Cars in Hagen.—*E. T. Z.*, 1895, No. 3, p. 37.

ANON.—Electric Suspended Railway—Langen System.—*E. T. Z.*, 1895, No. 3, p. 45.

MAGNETISM.

E. ODDONE—On Variations of the Intensity of Magnetisation in Magnetic Rocks.—*Jour. de Phys.*, Dec., 1894, p. 575.

E. VILLARI—Action of Transverse Magnetisation on the Ordinary Magnetism of Iron and Steel.—*Ibid.*, p. 575.

D. HURMUZESCU—Electro-motive Force of Magnetisation.—*C. R.*, vol. 119, No. 23, p. 1006.

J. B. HENDERSON—Influence of Magnetisation and Temperature on the Electrical Conductivity of Bismuth.—*W. A.*, vol. 53, No. 13, p. 912 (I.).

J. KLEMENČIČ—On Self-Induction in Iron Wires.—*Ibid.*, p. 1053.

T. MOUREAUX—On the Absolute Value of the Magnetic Elements on January 1st, 1895.—*C. R.*, vol. 120, No. 1, p. 42.

G. FOLGHERAITER—The Source of the Magnetism in the Volcanic Rocks of Latium, and its Distribution.—*Beibl.*, vol. 19, No. 1, p. 100.

F. S. DE TOUCHIMBERT—Observations on the Daily Variations of Declination of the Magnetic Needle.—*C. R.*, vol. 120, No. 3, p. 170.

C. FROMME—On the Self-Induction and Electrostatic Capacity of Coils of Wire, and their Influence on Magnetic Phenomena.—*W. A.*, vol. 54, No. 1, p. 1.

M. WEBER—On Electro-magnetic Pulling Force.—*Ibid.*, p. 30.

INSTRUMENTS AND MEASUREMENTS.

J. ANIZAN—The Mercadier and Anizan Microphone.—*Ecl. El.*, vol. 1, No. 15, p. 677 (I.).

F. L. O. WADSWORTH—Description of a very Sensitive Form of Thomson Galvanometer, and some Methods of Galvanometer Construction.—*Phil. Mag.*, vol. 38, No. 235, p. 553 (I.).

G. MEYER—The Capillary Electrometer and Drop Electrodes.—*W. A.*, vol. 53, No. 13, p. 845.

M. WEIN—On the Calculation and Measurement of Small Self-Inductions.—*W. A.*, vol. 53, No. 13, p. 923 (I.).

K. SCHERING and C. ZEISSIG—New Photographic Method of Registration of the Position of the Magnet in Magnetometers and Galvanometers.—*Ibid.*, p. 1039 (I.).

M. T. Edelmann—A Large Wheatstone's Bridge.—*E. T. Z.*, 1894, No. 51, p. 698 (I.).

Anon.—Watertight Electric Bell.—*E. T. Z.*, 1894, No. 52, p. 717 (I.).

M. Sala—The Electric Conductivity of German Silver in different Media.—*Beibl.*, vol. 19, No. 1, p. 80.

TELEGRAPHY AND TELEPHONY.

E. Baudot—Multiplex Telegraphy.—*Bull. Soc. Int.*, vol. 11, No. 113, p. 530.

Anon.—The Official Vocabulary of Words admissible for Telegraphic Correspondence in Conventional Language.—*Jour. Tel.*, vol. 18, No. 12, p. 345.

Anon.—Telegraphs in Brazil in 1892.—*Ibid.*, p. 347.

Anon.—Telegraphs in the Dutch Indies in 1892.—*Ibid.*, p. 350.

Anon.—Telegraphs in British India, 1892-94.—*Ibid.*, p. 353.

C. Barnes—Telephonic Measurement of Electro-motive Force.—*Phil. Mag.*, vol. 38, No. 235, p. 558.

Anon.—The 1894 Cable of the Anglo-American Telegraph Co.—*E. T. Z.*, 1894, No. 49, p. 666 (I.).

Anon.—The Berlin-Vienna Telephone Line.—*E. T. Z.*, 1894, No. 50, p. 692.

Anon.—System of Connection for Telephone Stations.—*E. T. Z.*, 1894, No. 51, p. 699 (I.).

W. Finn—The Wheatstone Printing Telegraph in the United States.—*E. T. Z.*, 1894, No. 52, p. 710.

W. Œsterreich—New Apparatus for Domestic Telegraphy and Telephony.—*Ibid.*, p. 711 (I.).

Anon.—The Telegraphs in British India, 1892-94.—*Jour. Tel.*, vol. 19, No. 1, p. 7.

Anon.—Telegraphs and Telephones in Spain in 1892.—*Ibid.*, p. 14.

Anon.—Telegraphs in Bolivia in 1894.—*Ibid.*, p. 18.

— Piérard—On the Nature of Telephonic Perturbations produced in Overhead Lines in the Vicinity of Conductors carrying Variable Currents.—*Ecl. El.*, vol. 1, No. 3, p. 121 (I.).

C. von Barth—A Technical Contribution to the Solution of the Question of Telephone Tariffs.—*E. T. Z.*, 1895, No. 4, p. 53.

ELECTRO-CHEMISTRY.

S. Lussana—Influence of Magnetism and Heat on the Movement of Ions.—*Jour. de Phys.*, Dec., 1894, p. 578.

S. Lussana—Experimental Researches on the Thermo-electric Qualities of Electrolytes.—*Ibid.*, p. 579.

S. Lussana—Electrical Resistance of Saline Solutions near their Maximum Density.—*Ibid.*, p. 580

E Andreoli—Extraction of Gold by Cyanide.—*Ecl. El.*, vol. 1, No. 14, p. 637.

Anon.—The Electric Sanitation of Houses by the Hermite System.—*Ibid.*, p. 644

Anon.—The Caldwell Electrolyser.—*Ibid.*, p. 658 (I.).

F. Sandford—Some Experiments on Electric Photography.—*Ibid.*, p. 665.

Anon.—Electric Bleaching.—*Ecl. El.*, vol. 1, No. 15, p. 692.

Anon.—Apparatus for the Refinement of Silver and other Metals.—*Ecl. El.*, vol 1, No. 15, p. 693 (I.).

H. Moissan—Further Researches with the Electric Furnace.—*C. R.*, vol. 120, No. 1, p. 17.

J. Garnier—Action of the Electric Current on a Series of Metallic Sulphides in a State of Fusion.—*C. R.*, vol. 120, No. 4, p. 184.

Anon.—The Carmichael Electrolyser.—*Ecl. El.*, vol. 1, No. 1, p. 33 (I.).

Anon.—The Killiani Diaphragms.—*Ibid.*, p. 35.

Anon.—A New Product of the Electric Furnace.—*Ecl. El.*, vol. 1, No. 3, p. 126.

S. Lussana—On the Thermo-electric Power of Electrolytes.—*Ibid.*, p. 139.

Werner and Miolati—Contributions to the Theory of the Constitution of Inorganic Compounds.—*Beibl.*, vol. 19, No. 1, p. 80.

C. Schall—On the Decrease of the Molecular Conductivity of certain Strong Organic Acids on replacing the Water of the Solution by Alcohol.—*Ibid.*, p. 82.

W. F. Knox—On the Conductivity of Aqueous Solutions of Carbonic Acid.—*W. A.*, vol. 54, No. 1, p. 44.

ACCUMULATORS.

Anon.—Preparation of the Active Material for the Barbier Accumulators.—*Ecl. El.*, vol. 1, No. 14, p. 657 (I.).

Anon.—The Chloride of Lead Accumulator.—*Ecl. El.*, vol. 1, No. 2, p. 74 (I.).

STATIC AND ATMOSPHERIC ELECTRICITY.

R. Arno—Rotating Electric Field, and Rotations produced by Electrostatic Hysteresis.—*Jour. de Phys.*, Dec., 1894, p. 574.

— Vaschy—On the Electrostatic Capacity of a Line traversed by a Current.—*C. R.*, vol. 119, No. 26, p. 1198.

R. A. Fessenden—Note on the Measurement of the Specific Inductive Capacities of Water, Alcohol, &c.—*Phil. Mag.*, vol. 38, No. 235, p. 567.

Wiedemann and Schmidt—On Formation of Ozone under the Influence of Electric Oscillations.—*W. A.*, vol. 53, No. 13, p. 924 (I.).

B. Thwing—On a Relation between Specific Inductive Capacity and the Chemical Constitution of Dielectrics.—*Ecl. El.*, vol. 1, No. 2, p. 85.

R. Arno—Electrostatic Rotations in Rarefied Gases.—*Ecl. El.*, vol. 1, No. 3, p. 138.

R. Arno—The Retardation of Polarisation in Dielectrics.—*Ecl. El.*, vol. 1, No. 4 p. 160.

C. V. Zenger—Solar Activity and Aurora Borealis.—*Ecl. El.*, vol. 1, No. 4, p. 163.

A. Campetti—On the Determination of Dielectric Constants by means of Rapid Oscillations.—*Beibl*, vol. 19, No. 1, p. 78.

Le Royer and v. Berchem—Measurement of Wave-Length of a Hertz Exciter in Air by means of the Alterations of Resistance of Metallic Filings.—*Ibid.*, p. 93

THEORY.

R. Mewes—The Relation between the Electrostatic and Electro-magnetic Systems of Units on the Vibration Theory.—*E. T. Z.*, 1894, No. 52, p. 712.

C. E. Guillaume—Application of the Principle of Döppler to the Energy of Radiation.—*Jour. de. Phys.*, Jan., 1895, p. 24.

— Vaschy—On the Law of Transmission of Energy between its Source and the Conductor in the case of a Steady Current.—*C. R.*, vol. 120, No. 2, p. 80.

J. Blondin—On the Propagation of Electric and Magnetic Disturbances.—*Ecl. El.*, vol. 1, No. 2, p. 62, No. 3, p. 110 (I.).

W. Lahmeyer—The Regulation of Polyphase Current Circuits, and of Polyphase Direct-Current Transformers.—*Ecl. El.*, vol. 1, No. 2, p. 72.

R. Arno—On the Law of Loss of Energy in Dielectrics under the Influence of Weak Electric Fields.—*Beibl.*, vol. 19, No. 1, p. 77.

VARIOUS.

A. Battelli—On the Variations of Electric Resistance and Thermo-electric Qualities in Nickel with Alterations of Temperature.—*Jour. de Phys.*, Dec., 1894, p. 578.

G. Vincentini—Transmission of Electricity by the Air surrounding Conductors Heated to Redness by an Electric Current.—*Ibid.*, p. 580.

J. Violle—On the Temperature of the Electric Arc.—*C. R.*, vol. 119, No. 23, p. 949.

G. G. de Villemontée—Electric Potentials in a Conducting Liquid in Uniform Movement.—*C. R.*, vol. 119, No. 26. p. 1201.

P. Curie—On the Symmetry of the Magnetic Field and of the Electric Field.—*Ecl. El.*, vol. 1, No. 14. p. 627.

C. Henry—Pupillometry and Photometry.—*Ecl. El.*, vol. 1, No. 15, p. 673.

K. Noll—Thermo-Electricity of Chemically Pure Metals.—*W. A.*, vol. 53, No. 13, p. 874.

Anon.—Proposals of the Technical Committee of the Electrotechnische Verein relating to the Rules for Protection against Fire with Electrical Installations —*E. T. Z.*, 1894, No. 51, p. 699.

J. de Kowalski—On the Production of Cathode Rays.—*C. R.*, vol. 120, No. 2, p. 82 (I.).

Anon.—Aluminium Utensils for Domestic Use.—*Ecl. El.*, vol. 1, No. 2, p. 77.

K. E. F Schmidt—The Importance of Faraday's Conception of Lines of Force for the Early Steps in the Teaching of the Theory of Electricity.—*Beibl.*, vol. 19, No. 1, p. 79.

F. J. Smale—Researches on Gas Cells.—*Ibid.*, p. 84.

Anon.—German Electrical Business in the Year 1894.—*E. T. Z.*, 1895, No. 1, p. 11, No. 2, p. 29. No. 3, p. 40.

H. Görges—Comparative Remarks on the Utility of the Single and Polyphase Systems.—*E. T. Z.*, 1895, No. 3, p. 46.

K. Feussner—Cable Points.—*E. T. Z.*, 1895, No. 5, p. 66 (I.).

O. Frölich—On the Price of Ozone.—*E. T. Z.*, 1895, No. 5, p. 67.

P. Janet—On an Electro-chemical Method of Recording Alternating Currents.—*Bull. Soc. Int.*, vol. 12, No. 114, p. 6 (I.).

R. Arnoux—Elastic Coupling and Phenomena of Resonance.—*Ibid.*, p. 23.

V. Bjerknes—Different Forms of Multiple Resonance.—*W. A.*, vol. 54, No. 1, p. 58.

O. Wiedeburg—The Laws of Surface Tension of Polarised Quicksilver.—*Ibid.*, p. 64.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXIV. 1895. No. 116.

The Two Hundred and Seventy-second Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 14th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting of Thursday, January 24th, 1895, were read, and the names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Percy Valentine McMahon.

From the class of Students to that of Associates—

Percy Harry Budgen.
Henry Charles D. Fearon.
Gerald Douglas Gibson.
John George M. Hilton.
George Hoser.
Charles E. Tickner.

Mr. C. E. Grove and Mr. F. V. Andersen were appointed scrutineers of the ballot.

The following paper was then read :—

REVERSIBLE REGENERATIVE ARMATURES AND SHORT-AIR-SPACE DYNAMOS.

By W. B. SAYERS, Associate.

Mr. Sayers. The subject of the present paper is the development of the commutating device for continuous-current dynamos which I had the honour of bringing under your notice two years ago, and which has since been put into practice to a considerable extent by Messrs. Mavor & Coulson, of Glasgow, and other firms.

It will be remembered that in my former paper I described a machine the armature of which generated its own field, the usual field magnets being replaced by simple [-shaped iron blocks, which Lord Kelvin, on viewing the machine, at once appropriately called "keepers." This property of a generator armature, when commutated on the backward lead, of producing or augmenting, as the case may be, the effective magnetic flux may, I think, be correctly described as a regenerative property, and I therefore propose to call armatures capable of being so commutated "regenerative armatures."

I may here state what are the advantages secured by the use of the devices about to be described.

1. The commutation of slotted, or tunnel, armatures—which, it is generally admitted, are very desirable from a mechanical point of view, but the iron-embedded coils of which necessarily have a high coefficient of self-induction—is successfully performed, and that with the shortest permissible air space and minimum exciting power in the field.

2. Not only may such armatures be successfully commutated, but, speaking of generators, they may be commutated on the backward lead; and when this is done, the magnetic circuit may be the shortest possible (and consequently the lightest possible), while still allowing sufficient space for shunt winding, the forward turns on the armature securing an approximately level, or even, when desired, a rising characteristic, the shunt only requiring to

be sufficient to give the initial field required for the light load E.M.F. Mr. Sayers.

3. It is found practicable to commutate very heavy currents without having resort to duplex or triplex winding and consequent many part commutators, or of going to four or more poles for large machines; and this, coupled with the fact that the iron-embedded conductors do not require to be laminated, renders large armatures on my plan not more, but rather less, expensive to build than the older types, thus doing away with the main objection which has been raised to the use of commutator coils, *i.e.*, that the cost of the armature is increased by their use. As an example, a machine will be found described later on having only 36 parts in commutator; main conductors of solid bar, almost lightest possible magnetic circuit; the output being 800 amperes and 100 volts at 420 revolutions per minute, the combined efficiency of engine and dynamo being 87 per cent.

Reversible Regenerative Armatures.

It was found that in many cases it was a serious drawback to machines constructed with regenerative armatures on my plan that they could only be run in one direction; in other words, the device was not reversible without structural alterations in the armature. I found a solution to this difficulty, however, in the construction shown in Figs. 1 and 2. In these figures A_1 and A_2 represent two sections of the main winding of a drum armature. *b* is a commutator coil, which is connected to the main winding at the junction, A_3, between coils A_1 and A_2. C is a commutator segment. The direction of winding is the same in the two figures, but in Fig. 1 the direction of rotation is represented as being clockwise, while in Fig. 2 it is counter-clockwise. A little study of these two diagrams will show that the commutator coil acts in precisely the same way in whichever direction the armature rotates, the brushes being always set with a backward lead, so as to bring the side of the commutator coil, B_1 or B_2, as the case may be, which is behind the point of connection (A_3) to the main winding under the edge of the flux horn. It will be noticed that in both cases only one side of the commutator coil *b* is

Mr. Sayers. operative, the other side being in a position in the interpolar gap where normally there is no effective field.

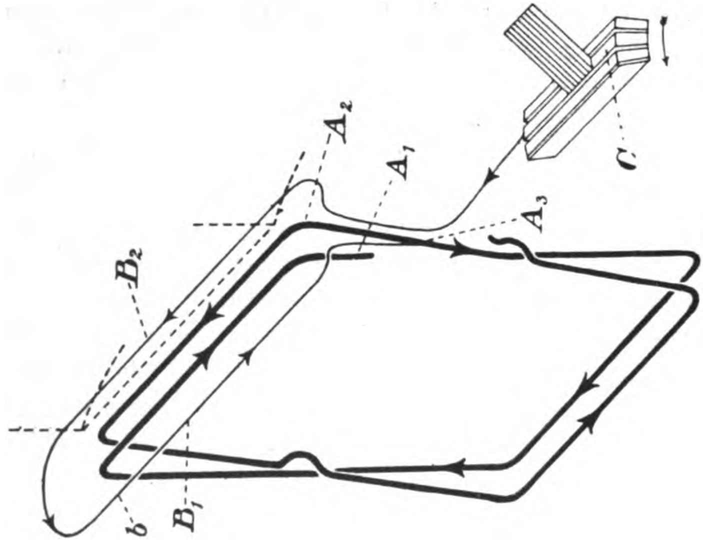


Fig. 2.

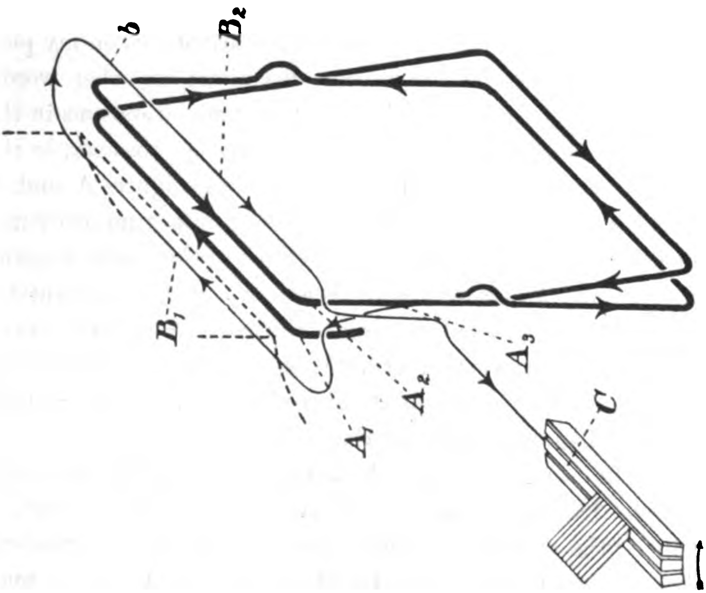


Fig. 1.

Elements of Reversible Winding.

I do not propose on this occasion to take up time with a minute description of the action of this device, the action of the

reversible form now described being substantially the same as the simple form described in my earlier paper. In a few words, however, I may say the effective part of the commutator coil is still well under the flux horn when the commutator segment to which it is connected comes into contact with the heel of the brush, while the commutator coil immediately preceding it, which is under the brush and momentarily carrying the full current, is just passing out of the field, and consequently is the seat of a weaker and rapidly falling E.M.F.; the result of which is that the current flowing from the main winding is rapidly transferred to the more backward coil which is still in the strong part of the field and so is the seat of maximum E.M.F. In this way a third commutator segment comes under the heel of the brush, the action already described is repeated, and so the commutation proceeds. (See Fig. 9.) Mr. Sayers.

I do not know whether it has been generally noticed that a close analogy exists between the problem of sparkless commutation and the problem of preventing knocking in a high-speed engine. In the latter case the valve must have a certain amount of lap and lead in order to give a sufficient compression and put on the live pressure at the proper time to just bring to a stand the motion of the connecting-rod, piston, &c., at the moment the crank is on the dead centre, and to restart them in the opposite direction. Just as the inertia of the reciprocating parts of the engine must be overcome by the steam pressure, and not by the crank action, so the reversal of the current-direction of the armature sections against the self-induction (the electrical analogue of inertia) must be overcome by internal electro-motive forces, and on no account by the make-and-break action of the commutator segments and brushes. I think the analogy between these two things is strikingly complete.

Auxiliary Devices.

In the simplest form of reversible machine only half the length of the commutator coil is effective in producing the reversal when the armature is running in a given direction. Fig. 3, however, shows a means of bringing both sides of each coil

Mr. Sayers.

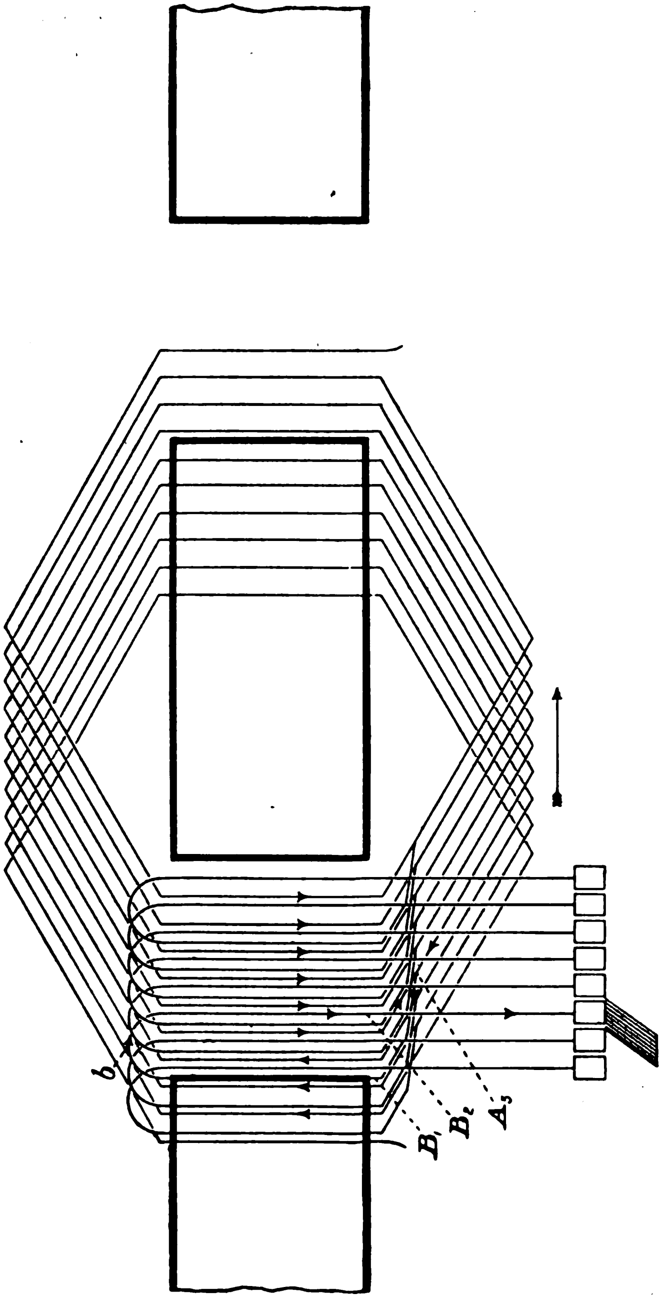


FIG. 9.—Diagram of Winding, with One Commutator Coil to each Turn of Main Winding.

into operation. P is a pole-piece forming an extension of the flux horn, leaving a recess having an angular breadth approximately equal to, or a little greater than, the angular width of each Mr. Sayers

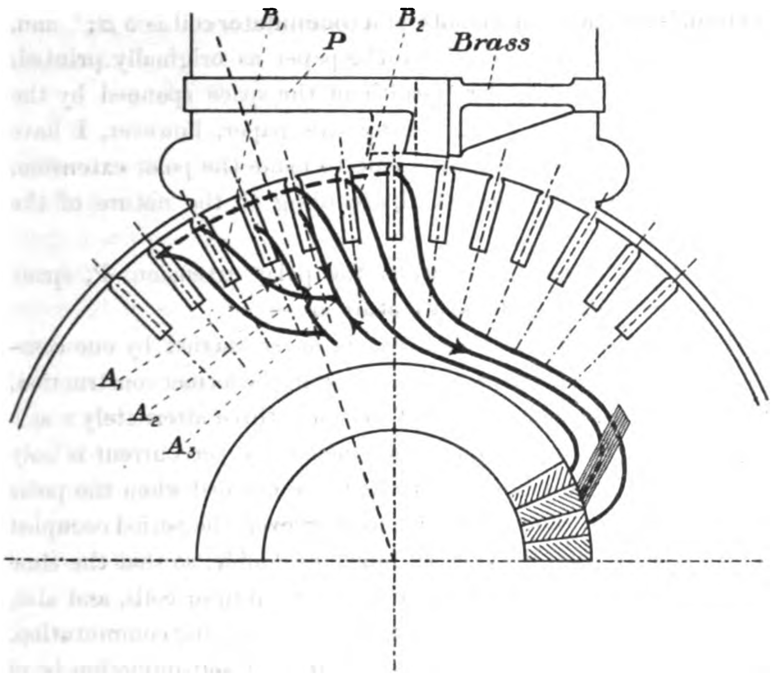


FIG. 3.

commutator coil. The commutation is effected in this recess, the section of main winding being located about midway in the recess at the time when the current in it is changing. It will be seen that the same action as has already been described takes place with regard to the part of the commutator coil which is behind the plane of commutation; but, in addition to this, the forward part of the coil next in front—in which the current is falling—has come under the polar extension, and as the field under this extension is of the same direction as under the main portion of the polar surface, to which it is magnetically connected, while the coil is cutting it in an opposite sense, this coil is the seat of a back E.M.F. which operates against the current in this coil, and so assists in producing the reversal of the main armature section,

Mr. Sayers. which is in series between the two commutator coils under consideration. In Fig. 3 it will be noticed that the polar extension, P, is shown in full, spanning four teeth—that is, one tooth more than the number embraced by a commutator coil. Using Dr. Thomson's notation, the breadth of a commutator coil is 3 β;* and, according to what I have said in the paper as originally printed, this should be the angular breadth of the space spanned by the polar extension, P. Since writing this paper, however, I have found it to be a great improvement to make the polar extension, P, span $3 + 1\ \beta$ or $2{\cdot}5 + 1\ \beta$, according to the nature of the winding.

The advantages secured when the polar extension, P, spans this additional extent of the armature are—

1. The whole armature current is never carried by one commutator coil, as it is for part of the time in the former construction, but is divided between two and between three alternately: as a consequence, the maximum value reached by the current is only about half as great compared to the value reached when the polar extension has the shorter span; and, moreover, the period occupied by the rise and fall may be as much as double, so that the time rate of change of the current, both in commutator coils, and also, of course, in the section of main winding undergoing commutation, is greatly reduced. The opposing E.M.F. of self-induction is, of course, correspondingly reduced, so that a much heavier current may be sparklessly commutated when the polar extension is made to span the greater breadth.

2. When the span of the polar extension is equal to the angular width of a commutator coil, the bearing surface of the brush must not be greater than the width of one commutator segment; but when it is made to span the greater width the brushes may cover anything between one and two segments, so that the length of commutator may be reduced without diminishing the area of surface in contact between the commutator and brushes.

In designing machines with a polar extension such as I have described, it must be borne in mind that, as the flux conveyed by

* Dr. Thomson uses β to signify the angular breadth of a section of armature winding or of a segment of commutator.

the extension, P, enters the armature at a point past the plane of commutation, it is non-effective in generating E.M.F. as it enters and leaves the armature on the same side of the plane of commutation; it must therefore be treated as leakage, and provision made for this non-effective flux in the cross section of the field magnets. Mr. Sayers

Number of Segments in Commutator.

The necessity hitherto found for using a large number of sections in the commutator, and of resorting to duplex or triplex winding in the case of large low-tension machines, ceases to exist when machines are designed on the lines I am describing, and the number of segments is governed practically by the difference of potential it is advisable to allow between adjacent commutator segments. Of course it would generally be inadvisable to reduce this number sufficiently to make the current undulatory to an appreciable extent, unless for machines for arc lighting.

That nothing is gained by using a large number of sections in the commutator is easily found from theoretical considerations, and is borne out by experience. Consider a two-pole drum armature having 24 coils in series generating the E.M.F.—that is, a total of 48 coils. If we use a 48-part commutator, the time occupied in the reversal of an armature section will be 1-48th of the time of one revolution. If, however, we use a 24-part commutator, the time occupied will be 1-24th. The reversing E.M.F. will therefore be acting for twice the time in the latter case, as compared with the former. In addition to this, in the latter case the angular distance between successive commutator coils is doubled, so that the one which is going out of circuit is farther away from the pole-tip, and so well out of the fringe; so that the reversing E.M.F. (which is always the difference between that generated in the coil going out of circuit and the coil coming into circuit under the brush) is greater with the lesser number of segments. And, again, whereas in the first case the opposing self-induction is that of one main section plus two commutator coils, in the second it is only two main sections plus *two* commutator coils—that is, less than double; and then, most important of all, the rate of change is only one-half in the latter case of what it would be in the

Mr. Sayers. former. The reduction of the number of bars in the commutator has the further advantage that it allows of the brushes bearing on a larger arc of the commutator surface, so that a smaller width of brush is necessary. It would not do, however, to reduce the number of slots, or tunnels, in the armature, and put two coils in each instead of one, as this would have the effect of increasing the self-induction four times.

Design of Slotted, or Tunnel, Armatures.

Just as the steam engine designer makes his reciprocating parts as light as possible consistent with strength, so it is worth while to so design the slots, or tunnels, in an armature as to get

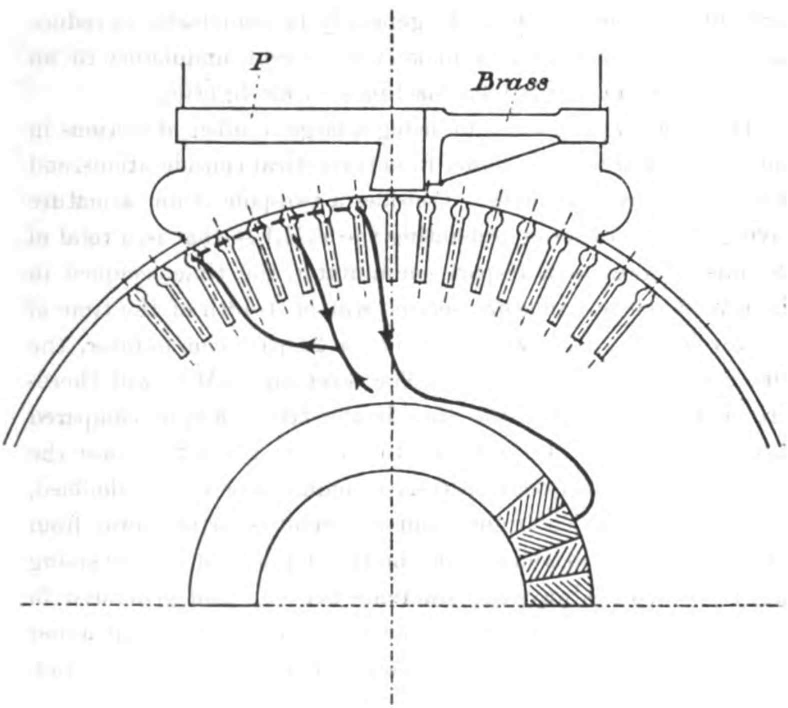


Fig. 4.

the least amount of self-induction; and this is to some extent antagonistic to other desirable features in short-air-space machines.

Mr. Sayers.

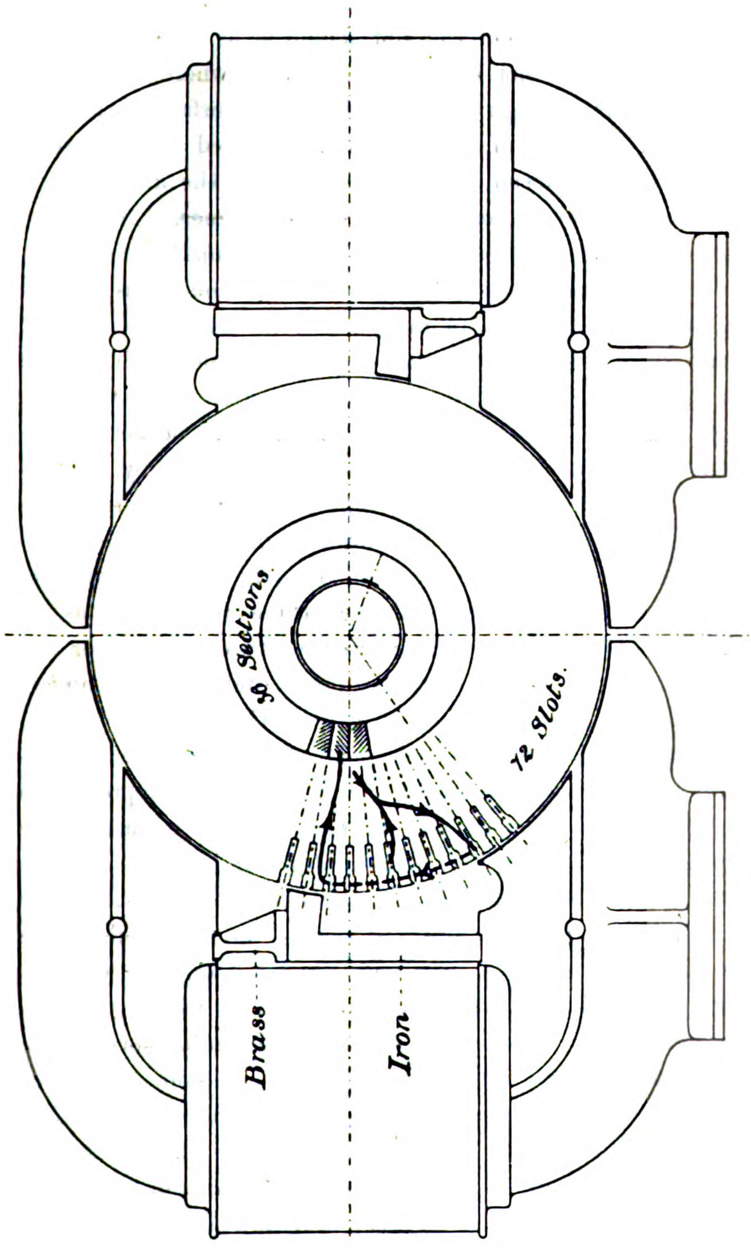


FIG. 5.—80-K.W. Dynamo, 400 revs. Constant, 200.

Mr. Sayers. In the first place, in order to keep down the self-induction, the opening at the top of the slot should be as wide as possible consistently with its not exceeding the limit where appreciable loss would occur through eddies in the polar surface of the field magnets. The width of this opening, as stated in my former paper, should not exceed one and a half times the air space. I usually make it about one and a quarter times. Next, the number of conductors in each slot should be as small as consistent with considerations of cost of manufacture; though, as already shown, it is advantageous in many cases to make the number of commutator segments as small as possible—the conductors in two or more pairs of slots forming but one section of the winding. In Figs. 3, 4, and 5 are represented some designs of slots which have given satisfactory results in machines made by Messrs. Mavor & Coulson. In Fig. 3 is represented a plain slot with reversible commutator coils, the number of segments in commutator being equal to the number of slots in the armature core. The commutator coil embraces three teeth, or has an angular breadth of 3 β. There are thus two commutator coil wires and two main armature conductors in each slot; of the two former, one only is in operation at a time, the second belonging to a commutator coil further back under the pole, and which has not yet come into circuit under the brush. Fig. 4 represents a slot having a groove planed near the top on each side into which the round wire forming the commutator coil is slid. In this design the number of commutator segments is half that of the slots, and the commutator coils half that of the main coils. The commutator coil embraces five teeth, and its angular width is thus $2\frac{1}{2}$ β, there being two convolutions of main winding to each section, but each convolution wound in separate slots The alternate wires come into operation in succession, the wires intermediate between these belonging to commutator coils further back, and not in circuit for the time being. This design, in addition to advantages already pointed out, permits of the armature being a little smaller in diameter, and also requires no binding, the commutator coils effectively securing the main conductors in their position in the core. Fig. 10 is a diagrammatic

Mr. Sayers.

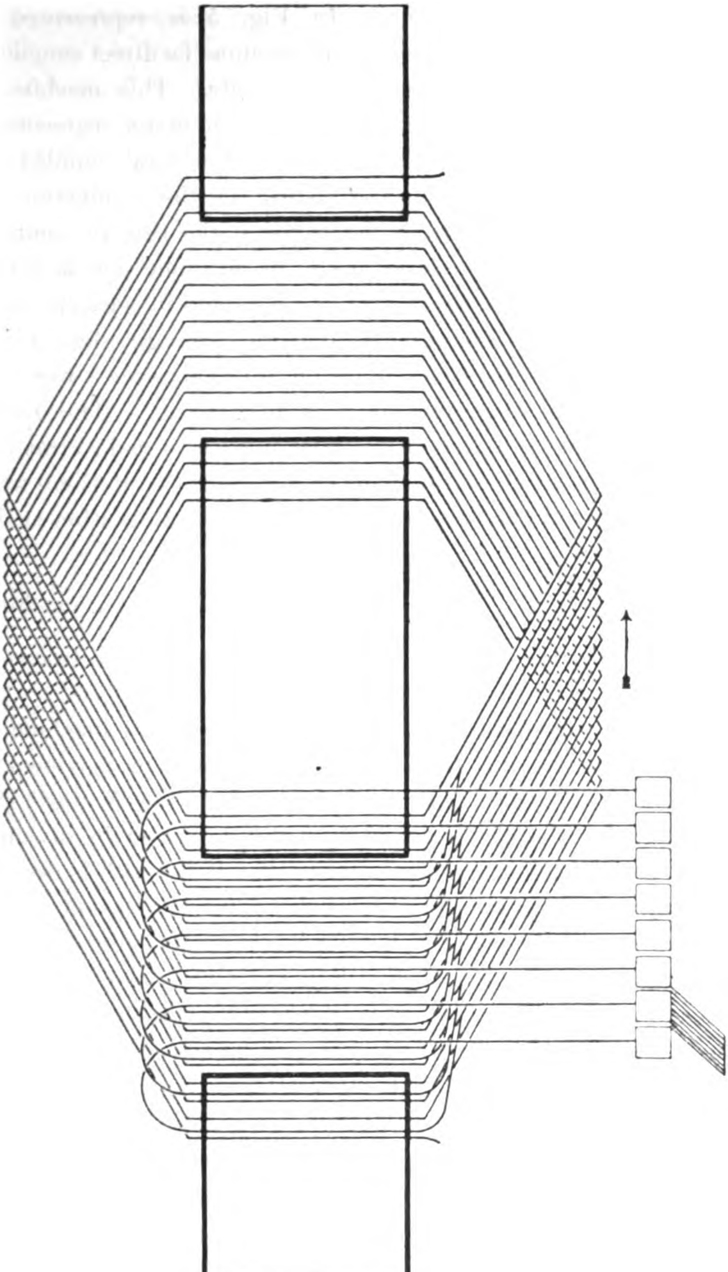


Fig. 10.—Diagram of Winding, with One Commutator Coil for every Two Main Convolutions.

Mr. Sayers representation of the winding. In Fig. 5 is represented a construction adopted in an 80-kilowatt machine for direct coupling, the speed being 420 revolutions per minute. This machine I shall describe later on. The number of commutator segments is one-half that of the slots, but equal to the total number of convolutions of main winding. Instead of the conductors at opposite potentials—*i.e.*, those connected at the time of commutation to the opposite brushes—being in the same slots as is the case in the other two figures, these are wound in separate slots, each convolution of the winding passing through slots which are one removed from being diametrically opposite. Figs. 11 and 12 represent diagrammatically this winding. The result of this construction is that the self-induction of a main armature section is only about one-half of what it would be if there were two conductors in one slot, both undergoing commutation at the same time.

The quotient of $\frac{\text{Output in watts}}{\text{Revs. per minute}}$ gives the size of a machine. We have lately, at Messrs. Mavor & Coulson's works, taken to calling this the machine constant.

I find it very convenient in fixing the size of a machine to take as a basis the total flux, N, and this I make proportional to the square root of the machine constant. Having N fixed, the areas of all sections of the magnetic circuit are found by dividing N, plus the leakage allowance, by the value of B which is desired in each portion of the magnetic circuit. Thus the output of the machine is taken as being proportional, not to the third power of the lineal dimensions, but to the third power of the square root of the cross-sectional area at any given point in the magnetic circuit. Proceeding in this way, the preliminary calculations are not hampered by having to take into account the space occupied by the shaft, the depth of teeth, &c.

$$N = \sqrt{\text{machine constant}} \times 1{,}350{,}000.$$

The current which may be commutated in machines of the type I am describing is given by the empirical formula,

$$i_x = \frac{N}{l \times x \times n},$$

Mr. Sayers.

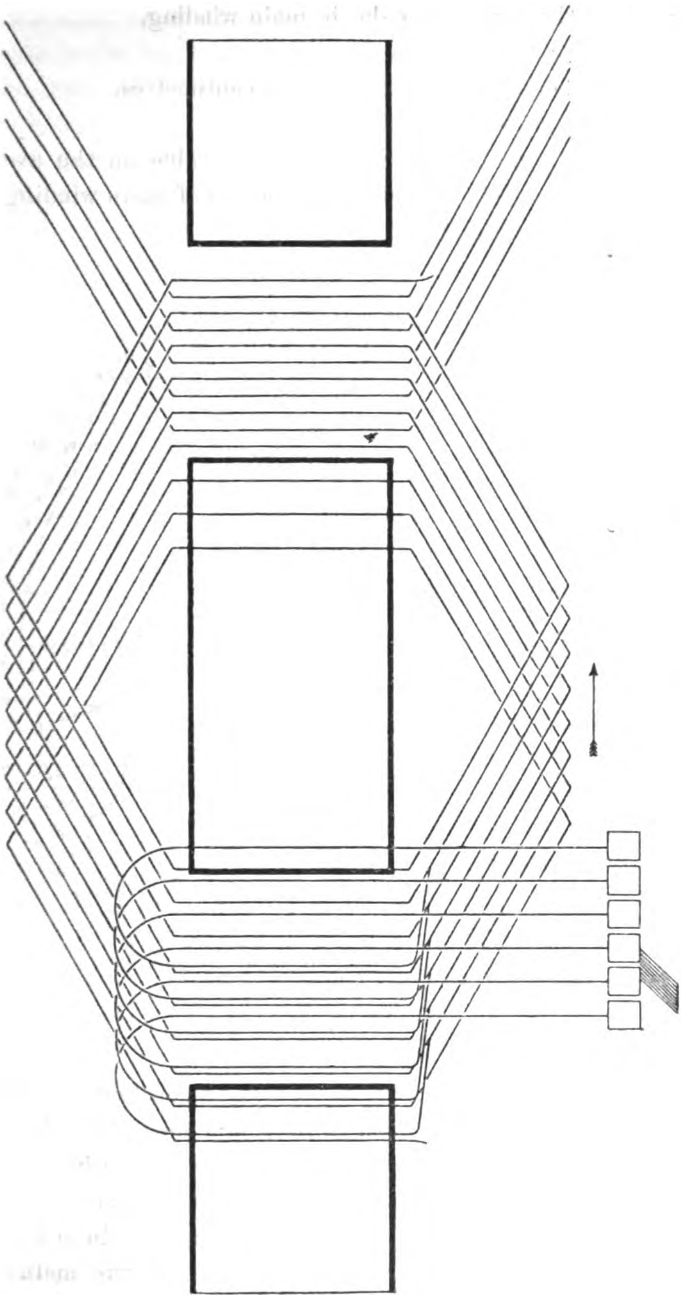


Fig. 11—Diagram of Winding of 80 K. W Dynamo.

Mr. Sayers. where i_x = the current per slot in main winding,

N = total flux per pole,

l = length of armature core in centimetres,

n = number of slots,

x = a variable depending for its value on the average coefficient of self-induction of main winding and commutator coils.

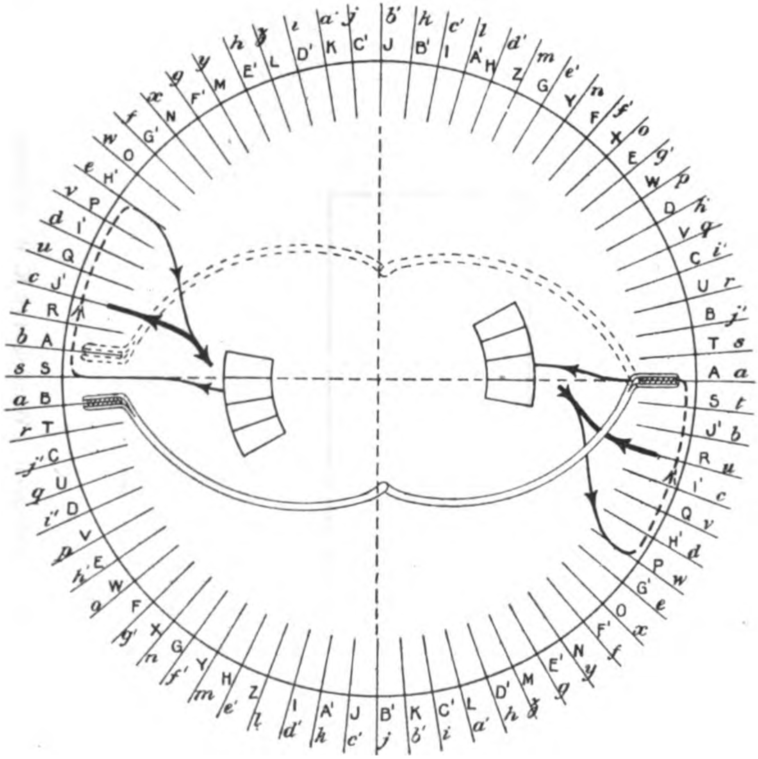


FIG. 12.—80 K.W. Dynamo.

In some 30-kilowatt machines having a constant of 39·5 the armature cores measuring 34·3 cms. × 34·3 cms., the value of x is 17; the form of slot being similar to that shown in Fig. 3.

In the 80-kilowatt machine herein described, having a constant of 200, the value is 13·5. The fact of the machine working sparklessly with this lower value of x is due to the method of winding, and to the larger opening at top of slot. The value of x

depends upon the form of slot, and especially on the width of the opening at the top. The coefficient of self-induction, L, is greater in the main conductors than in the commutator coils, owing to these being deeper in the slots, and greater also in the lower conductors than in those near the surface, for the same reason. I take the self-induction of the end connections as negligible compared with that of the portions of conductor embedded in slots. Mr. Sayers.

This empirical formula is, of course, capable of being developed into a scientific one by substituting the true value of L for the quantity x, and taking all the particulars of the winding, length of air space, and other conditions into account. The formula, however, is only independent of the speed at which machine runs, so long as the resistance of the conductors remains a negligible quantity, which it does throughout a considerable range.

PARALLEL RUNNING.

It is almost unnecessary to say that machines for running in parallel must not have a rising characteristic—at any rate, when combined with their driving power. I have made no experiment

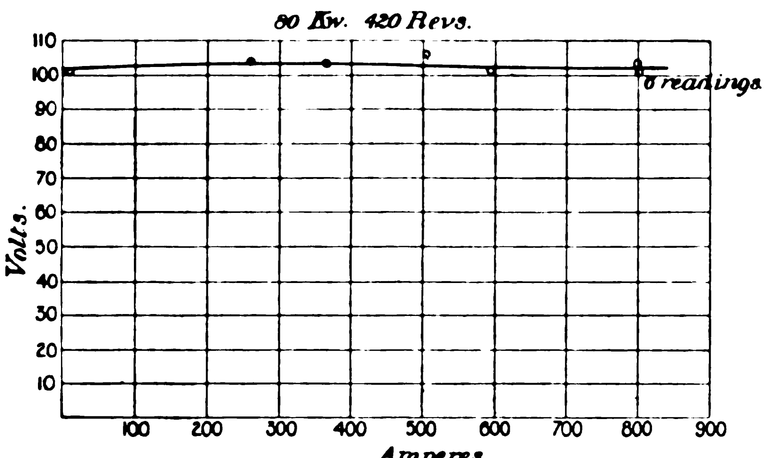


FIG. 6.

with direct-driven plants; but it is evident that if a combined plant running on the governors gives a falling characteristic, two or more such plants would run parallel, although the dynamo,

Mr. Sayers. when driven at absolutely *constant* speed, had a rising characteristic. Of course the characteristic is under the control of the designer, and, apart from questions of permeability of the magnetic system and relative magnetising powers of shunt and armature, depends upon the position of the plane of commutation relatively to the neutral plane.

Take Fig. 6, which is the characteristic of an 80-kilowatt machine when driven at a constant speed. This shows a rise up to 300 amperes, after which there is a gradual fall down to full load. If driven at a rigidly constant speed, or if coupled, or run from the same power supply, so as to both run at the same speed, two such machines would not run in parallel with safety. Fig. 7,

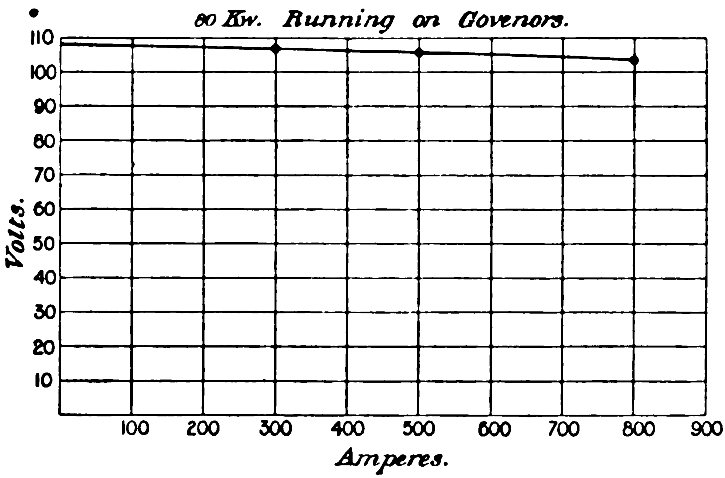


Fig. 7.

however, is the characteristic of the machine and engine combined, when the latter is running on its governors. The increase of speed with light loads is sufficient to transform the characteristic into a falling one, from which it results that two such separate plants would run in parallel perfectly. As I have already said, however, there is no difficulty about designing a machine to have a falling characteristic, even if driven at constant speed.

So far as my experience goes, there is not much inclination to run compound—by which I mean machines with a rising characteristic—machines parallel in this country; but if it were

desired to do so, the arrangement which suggests itself—a modification of the well-known method for machines with compound winding—is shown in diagram (Fig. 8). The brushes B B are Mr. Sayers

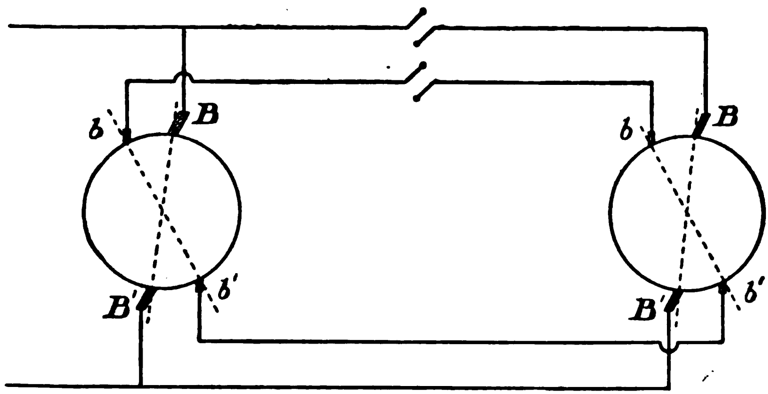


Fig. 8.—Proposed Arrangement for Coupling Machines with Rising Characteristic in Parallel.

the main ones, set with a backward lead, while *b b* are small subsidiary brushes, set on the forward lead. Suppose one machine were started, and it were desired to connect the second one in parallel, the second machine would be started and its E.M F. adjusted in the usual way; the small brushes, *b b′*, on the first machine would then be put down; next the two small, and after that the two sets of main brushes on the second machine. When connection is established between the two armatures at the four points *b′ b′* and B′ B′ on the same side of the commutators, part of the current traversing the coils between the two planes of commutation will leave the first armature by the small brush and flow through similar coils on the second armature, just as the current from the series coils on compound machines splits up between the two series coils when they are first thrown into parallel in the well-known arrangement. The other brushes may then be put down and switches closed, when the small brushes on the forward position will give stability to the system, in the same way as the independent connection to the brushes keeps compound machines from any tendency to play into each other. Although I have not tried this arrangement, I think there is no doubt that it would work satisfactorily if it were desired to run machines with a rising

Mr. Sayers. characteristic in parallel. It is usual, however, in central station work in this country to use shunt machines, and to control the current from each machine by means of a variable resistance in the shunt; and no doubt it will generally be preferred to use machines with a slightly falling characteristic for running in parallel, so as to avoid all complications. I may point out that with short-air-space machines considerable regenerative power is required in the armature in order to get even a moderate fall in the characteristic.

Rocking Brush-Holders for Reversing Motors.

For reversible motors the fact of the regenerative armature requiring a forward lead instead of a backward one, allows of a very simple and reliable automatic arrangement for shifting the brushes on reversal of direction of rotation. The rocking arm carrying the brush-holders is mounted on the shaft, and suitably bushed and lubricated so as to move freely; the friction of the brushes causes it to be carried round with the armature until brought up by stops which limit its travel in either direction. These stops are, of course, adjusted so as to keep the brushes in the proper position for sparkless commutation. Thus the position of the brushes is automatically changed without any sparking, as during the time in which the brushes are travelling from one position to the other immediately after a reversal they do not move relatively to the commutator, but travel round with it until brought up by the stops; so that there is no relative motion of commutator and brushes until the proper position is reached, and consequently no sparking (see Fig. 13).

Constant-Speed Motors.

The question has been put to me from more than one quarter whether motors fitted with regenerative armatures would be self-regulating—that is, run at constant speed with varying load when supplied with constant pressure at terminals—because the ordinary shunt motor owes its self-regulating qualities to the back turns in the armature, which have the effect of reducing the flux, and so compensating, so far as speed is concerned,

Mr. Sayers.

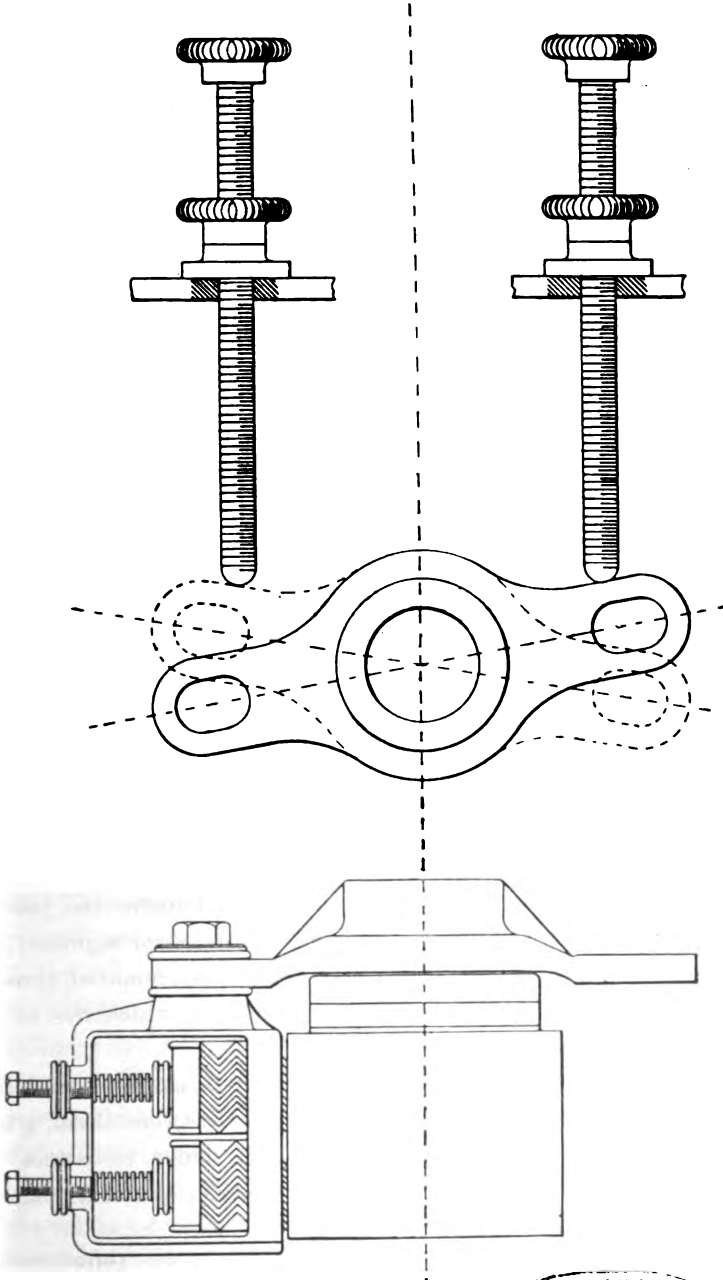


FIG. 18.—Automatic Rocker for Reversible Motor.

Mr. Sayers for the increased drop of pressure caused by dead resistance in the conductors internal and external to the machine. The reply to this question is, that while in the case of generators it may be inadvisable to reduce the air space below a given value on account of the crowding up of lines due to the large armature reaction, causing a diminution in the total flux, and so bringing down the characteristic, in the case of a motor this action can be taken advantage of and the air space reduced to a safe mechanical clearance: the reduction of the total flux due to crowding up will then tend to compensate for drop of pressure due to dead resistance. So that in the case of a motor we obtain the happy concurrence of lightest weight and minimum cost with best regulating qualities.

Description of 80-Kilowatt Dynamo built by Mavor & Coulson—420 Revolutions per Minute.

Armature.—Diameter over all	... ...	21 inches.
" core	... ...	$17\frac{1}{8}$ "
" hole in discs	... ...	$4\frac{1}{4}$ "

Total flux (effective), N = 20,600,000.

The main winding consists of 72 bars solid copper, 1·33 × 0·2 inch, connected at ends by "butterfly" connectors. Each bar is placed in a separate slot, and the connectors join the first to the thirty-sixth bar at each end. Thus the two bars in each convolution are one division less than diametrically opposite. The arrangement is that represented in Fig. 5. Referring to Fig. 5, the commutator, or reverser, bar which is just under the pole-tip is joined at the commutator end to the commutator segment. At the distant end it passes over to the slot seventh removed, through which it returns and is joined to the main bar, which lies in the slot third removed from it.

Magnets.—The magnets are of (so-called) cast steel, and each half is made in two parts, with a division between them, $\frac{1}{2}$ inch wide, parallel to the magnetic flow, in order to reduce the crowding up of the lines from cross induction, thus carrying out Professor Thomson's suggestion.

The polar angle is	... ...	120°.
The air-gap ...	... ...	$\frac{1}{4}$ inch.

The shunt winding consists of 325 pounds of No. 12 wire. Mr. Sayers.

Number of turns	2,016.
Resistance at 120° Fah.	10·7 ohms.
Current	9·35 amperes.
Ampere-turns...	18,800.*

The weights are as follows:—

	Cwts.	qrs.	lbs.
Magnets, steel castings, &c.	35	3	2
Magnet coils	2	3	17
Armature (shaft, core, and commutator)	20	3	15
Windings, main coils, reverser coils ...	4	3	12
Bearing, rocker, brush-holders, &c. ...	3	3	1

Fig. 6 is the characteristic of the machine running at constant speed.

Fig. 7 is the characteristic when the engine to which it was coupled was running on its governors.

The engine was built by Messrs. Belliss, of Birmingham, and their test showed an efficiency of $\frac{\text{E.H.P.}}{\text{I.H.P.}} = 86$ per cent.

While this test was being made, however, the shunt coils were coupled in parallel, with a resistance in circuit which dissipated 1 per cent. of the total energy. So that the real efficiency of the combined plant is 87 per cent.

The President. The PRESIDENT: I am sure, gentlemen, we have all listened with very great pleasure to Mr. Sayers, and I have to propose a vote of thanks to him for putting this interesting paper before us. The question is one of extreme importance to us all at the present moment; we all know that the object of a short air space design is to minimise the quantity of material necessary for the construction of dynamos and motors, and we know that in ordinary designs with short air space we have great trouble with the commutators and commutating arrangements generally. Mr. Sayers claims, and I believe justly so, to have overcome these difficulties, but up to the present it has not been an easy matter to make his machines reversible. We have now to thank him for the further development he has put before us this evening. I

* This means 9,400 ampere-turns in each limb of magnets.

The President. will ask you to pass a vote of thanks to Mr. Sayers for his paper, and then I shall be glad if the discussion will commence.

Mr. Andersen. Mr. F. V. ANDERSEN: I have not had much experience in my practice with short-air-space dynamos or with tunnel winding, and I am looking forward with some anxiety to seeing the performance of one of Mr. Sayers's machines. When you adopt tunnel winding you have a number of very difficult problems to solve—such questions as: the production of sparkless commutation with the increased self-induction; the amounts of increase in the losses in the machine; increase of hysteresis in the armature core caused by encasing the conductors, in which there is continual reversal of current, in iron, and increase of eddy-currents by introducing this kind of loss in the pole-pieces; further, the difficulty in cooling the encased armature conductors, —all these questions deter from preferring a tunnel-cored to a smooth-cored armature. I will not criticise the plan which Mr. Sayers has put forward for superseding the difficulties; but I should like to know whether any measurements have been made of the amount of hysteresis in his 80-kilowatt armature, and the eddy-currents in the field of that machine. There can be no doubt about the fact that if the troubles alluded to, which we do not find when we use smooth-cored armatures, can be successfully overcome with Mr. Sayers's regenerative armature, then his plan must be largely adopted, except in cases where shunt machines are required. Mr. Sayers, of course, cannot make a proper shunt machine; on his plan—*i.e.*, with the regenerative armature—the dynamo is virtually a compound machine, even when the field is shunt wound. Many of the largest machines now wanted must be shunt machines, when, namely, a number of machines are to run in parallel as one machine. I see in the paper that Mr. Sayers has a device for taking away the danger connected with compound winding on machines for parallel running.

Mr. SAYERS: May I point out that the machine which is shown in Fig. 5 is a shunt, but the suggested new arrangement for parallel winding is only for use when a machine is so designed as to give a rising characteristic.

Mr. ANDERSEN: Yes, but I mean the danger of a reversal

coming in on account of the regenerative action of the armature. Mr. Andersen.

Mr. SAYERS: I do not find that. I have some experience with that, and I do not find that even with a rising characteristic of the armature any actual reversal takes place, on account of the very small number of turns in the armature as compared with the field.

Mr. ANDERSEN: Of course the danger of reversal is removed by increasing sufficiently the power of the shunt over the magnetising power of the armature; but this will affect the saving of material due to Mr. Sayers's invention to a great extent. I wish to thank Mr. Sayers for the valuable information which he gives in his paper in regard to output obtained by him from given weights of machine at a given speed. It may perhaps be of some interest if I, in return, give a few figures taken from my own designs. These show a reason for my statement that there is an important saving to be had in the weights of machines if first-class results can be obtained with a Sayers armature. If you take the four first items on slip 4—*i.e.*, magnet castings, magnet coils, armature (shaft, core, and commutator), and armature winding—these four items weigh about 64½ cwt. That is the active material, so to speak, in the machine. But take the ordinary well-known single-horse-shoe machine for the same output at the same speed. If you make it a first-rate machine —sparkless, cool, and with an electrical efficiency of about 96 per cent., so that it will give just about such a commercial efficiency as Mr. Sayers has given in this paper for his machine—then the total weight of these four items, when the machine is economically designed, is about 85½ cwt. Therefore, with Mr. Sayers's machine, there is a gain in material of 24½ per cent. The gain is on the two first items only, and in the two last Mr. Sayers's machine is much the heavier of the two. The gain is about 40 per cent. in the cast steel; in the shunt wire it is 50 per cent., nearly. But Mr. Sayers's armature has some 34 per cent. more weight in the core and shaft, and some 21½ per cent. more weight in the winding, than the ordinary armature. In the labour I expect Mr. Sayers's machine is considerably the more expensive, as the

Mr. Andersen. larger armature, with its larger amount of copper and its great number of extra coils, must always tell heavily in the cost for labour. But, so far as the question is about compound machines, I have no doubt that there may be upwards of 10 per cent. saving in the cost of manufacture by making Sayers's machine in place of the single-horse-shoe machine. I am under the impression that Mr. Sayers's work includes the problem of working with the brushes fixed. This, of course, will be a very valuable point in his machine.

Mr. Mordey. Mr. W. M. MORDEY: I wish to take exception to two or three new terms that Mr. Sayers has introduced in this paper. We ought very jealously to watch the introduction of new terms. Although the new application of the word "keeper," mentioned in the second paragraph, has received the apostolic benediction of Lord Kelvin, I venture to think we ought not to accept it as a new name for field magnets. What is the origin of the term "keeper"? In early days it was found that ordinary magnets—steel magnets—would keep their magnetism longer if the poles were joined by a piece of iron; therefore that piece of iron was called a "keeper." There is no kind of analogy between that and the function which is played by the field magnets in Mr. Sayers's dynamo. It does not in any way make the magnetism more permanent.

Mr. SAYERS: It does not refer to that machine with a field winding, but to one in which the armature excited the magnets.

Mr. MORDEY: With or without any excitation applied directly to the field magnets, I think the term is not appropriate, and is not a good one. It does not make it a keeper any the more by having no winding on it. The term might equally well be applied to the yoke. Besides, the word is already very hard worked. I wish to object also to the proposed new use of the word "regenerative" as applied to armatures. It is not correct, and it is not expressive. If these new armatures must be distinguished from ordinary armatures, I do not think we can distinguish them in any better way than by calling them "Sayers armatures." I am sure Mr. Sayers has deserved that he should be distinguished by having his armatures called after him. Then there is another

term to which I object, and that is the use of the word "blades" for commutator sectors. We have been in the habit of calling these things "sectors." Perhaps they are not strictly sectors, as a sector comes down to the centre. They are truncated sectors, but there isn't time to say that. The word "blade" gives us an idea of a kind of Boadicea's car with sword-blades on the hub and the periphery, and it is altogether an objectionable term. The word "sector" satisfies all of us, and I hope it will satisfy Mr. Sayers. Mr. Mordey.

The main object of this paper, as I understand it, is to introduce to us the improvement by which Mr. Sayers has made his very clever arrangement suitable for machines that are required for running in either direction. We are very glad to know he has got over that difficulty. He has got over it by the plan of using twice the number and twice the space for the commutator coils. There is a point that he mentions in the paper, viz., the reduction of the number of sectors and of coils. He points out that with his arrangement it is not necessary to have a large number of sectors. Of course, in an ordinary machine we have been obliged, in order to avoid sparking, to have a great many sectors, and so to approximate to the condition of a brush running continuously along the wire. The condition nearest to this—that of an infinite number of sectors—is reached in Faraday's disc, and Forbes's and Ferranti's "unipolar" dynamos. Mr. Sayers points out that with his armature it is possible, on account of the ease with which the sparking is prevented, to reduce the number of sectors; and he intimates that we may go down very far in that direction. This is very important, and probably the reduction may be carried to a point where the pulsation of the current begins to cause trouble. But Mr. Sayers perhaps has omitted to notice that if you have a dynamo in which the E.M.F. is pulsating, you get an alternating current, which we all know is a very bad thing indeed. Now, Mr. Sayers's arrangement is not applied to the outside circuit; so that, if the outside circuit has any self-induction, there may perhaps be some sparking due to the reaction of the outside circuit.

Mr. Mordey. I must congratulate Mr. Sayers on the plain English of his paper. We are not used to having dynamo papers in which the author tells us in plain English what he means. I should like to ask Mr. Sayers, in conclusion, whether he is quite sure that that machine in the hall is a 30-unit machine.

Mr. Sayers: Oh dear, no!

Mr. Mordey: That is a great relief to me.

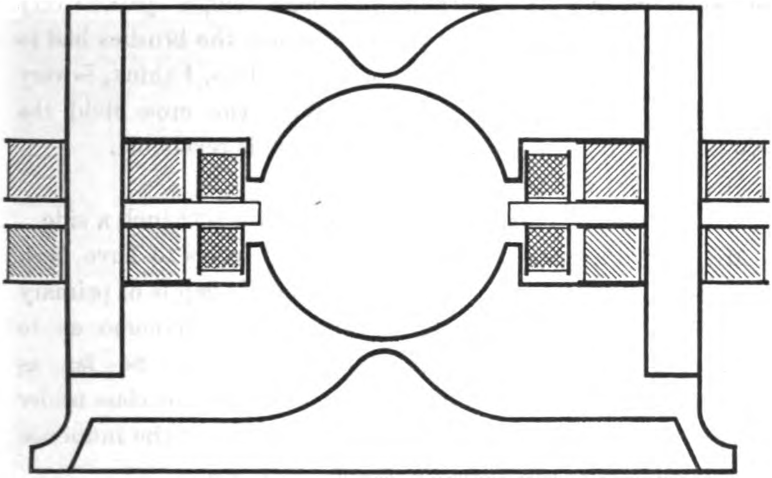
Mr. Mavor. Mr H. A. Mavor: I daresay it will interest you to have some remarks from me, as my firm has been mentioned in the paper as manufacturers of these machines. To begin with, I wish to give the fullest credit to Mr. Sayers for the energy he has shown in overcoming the difficulties which are necessarily incident to the introduction of so entirely new a design. We have great pleasure in standing by and assisting in the experiments—all the more pleasure that, as I am glad to say, not one of the machines has really turned out to be an experimental machine. That, I think, shows the possession by Mr. Sayers of what I might call the "rectifying eye," to make a parody on the "engineering eye;" because, of course, in the first instance the construction of these machines was not a question of calculation. Experiments had to be made, and, as he has told you, his formula on which he bases his practice now is an empirical one; and the fact that the empirical formula has been made up from experience in a large number of machines, all of which have ultimately turned out to be satisfactory, is rather remarkable. At first sight it looks as if it would be difficult to calculate all the operations that go on; but the device fortunately has the element of great flexibility in it, and so far it has proved perfectly successful in all the directions he has indicated. I think that in the paper he refers to a variety of forms of slots which have been used for these machines. I do not know whether the diagram referring to that has been omitted, but, so far as I can see, only two sections are shown. A great many forms have been tried, and all of them have given, so far, satisfactory results. With regard to the question of parallel running, raised by Mr. Andersen, I may mention that I had the opportunity of making some experiments with the 80-kilowatt machine represented in

Diagram 5; and, so far from experiencing any difficulty whatever in switching it into parallel, the expression used by the engineer in the small station where it was running was that it was "greedy "for the load." It took up the load very easily, and without any shock to the driving engine. It is evident to you that if the machine had a rising characteristic, and if it were switched into the circuit with other machines (always providing it is driven by an independent engine), if there is any tendency to reverse the other machines or increase the current in the machine in question, the engine is immediately pulled up, and the defect will remedy itself. In these machines, however, the forward turns are not very many, and the characteristic is practically horizontal. That as an experimental result will probably relieve the minds of some who feared that parallel running would be difficult with these machines. I may mention that the difficulty was brought prominently to our notice by the attempt to run two of those machines in parallel, driving off the same shaft. That was found to be impossible. The machines had a slightly rising characteristic. Fortunately, it was not necessary that they should be so used; but the difficulty quoted in that case might have been got over by the simple expedient of putting a turn or two of back-series winding on the magnets to bring down the characteristic. That was a very obvious way out of the difficulty, but it was not necessary to use it. With regard to the fixed position of brushes, that we have not quite arrived at in large machines yet; but we see our way to do so, and it is not very far off. The amount of motion required in the brushes of these machines is very slight indeed. On that 80-kilowatt machine, which is the largest we have constructed yet, the brushes require to be moved at half load, and then remain fixed up to the full load; and in the first position, from half load down to no load, simply self-exciting. With regard to the second formula on the board, I must say that I have not very fully thought out the matter; but, so far as I can see, the result would be identical if one took the induction as the numerator, and as the denominator used the ampere-turns on the machine, multiplied by two, giving the current-flux on the armature, and multiplying that by the length in centimetres of Mr. Mavor.

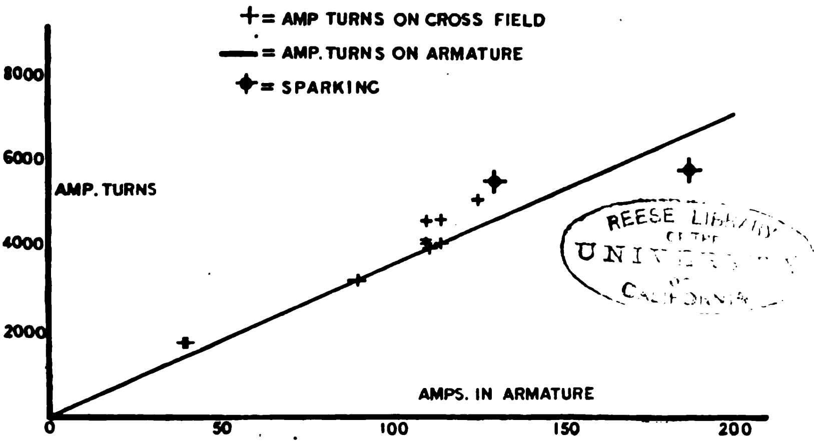
Mr. Mavor. the active conductor. Thus, $x = \frac{N}{2 \propto T \times l}$; Mr. Sayers's formula being $x = \frac{N}{i s \times n \times l}$. I agree with Mr. Mordey about the use of the word "regenerative." Regeneration means the being born again, and it does not apply in the present case. I suggest the term "direct flux," if Mr. Sayers does not care to register his name for posterity in connection with this armature. That, of course, would meet the case, and give a fair description of what the armature is. I have only, in addition, to thank Mr. Sayers for the paper.

Mr. Ravenshaw. Mr. H. RAVENSHAW: I think it is generally agreed that Mr. Sayers's invention is one of the best and most real of the last few years in connection with dynamo work, and I think great credit is due to him for it. At the same time, I must say that I think he puts his extra copper in the wrong place—he puts it on to his armature, which increases the complication of that part of the machine. Any continuous-current armature is, at the best, a very complicated affair, with a very large number of bars, a quantity of solder, as a rule, and tape and string and other things! It really is, from an engineer's point of view, a very unmechanical job; an alternating dynamo, as a rule, is very much simpler. Since Mr. Sayers read his first paper I have made a few experiments with what I have always known as a cross-field dynamo—that is to say, one with a non-sparking pole at right angles to the ordinary field. I have here a diagram of the machine, and a curve showing the result of the experiments. It was an ordinary notched armature machine, with 72 sections in the commutator and 72 notches. Armature, $10\frac{1}{2}$ inches diameter; core, 14 inches long; 100 volts, and 150 amperes. I altered the magnets slightly so as to get more room, and fitted a small electro-magnet in the middle of each magnet bar, pointing towards the centre, and having the same clearance as the ordinary pole-pieces. The cross-field dynamo was, I believe, originally due to Mr. Swinburne. It has been a well-known design, but I have always understood it was not really practicable. The diagram is to scale. I wound the cross-field coils with a small wire, so that they could

be separately excited. I put a certain amount of load on the machine, and then varied the current in the cross fields until the machine did not spark. I then increased the cross-field current, Mr. Ravenshaw



and the brushes began to spark; and then reduced it until they began to spark again, getting the mean readings which I have plotted on the diagram. The black line shows the actual ampere-



turns in the armature; the little crosses show the ampere-turns on the cross field. You will see they practically agree with the ampere-turns of the armature. The larger crosses and circles combined show two points where the machine sparked. One

Mr. Ravenshaw. current was too small, and another was too large. This is not a complicated arrangement. The cross coils are quite small, and the machine worked perfectly without sparking. As a matter of fact, when one worked it without this cross field, it sparked very little or not at all; but when the load varied, the brushes had to be constantly rocked to prevent sparking. This, I think, is very common in that type of machine. With the cross field the brushes are at right angles, and the machine is reversible.

Mr. Mordey: What is the clearance?

Mr. Ravenshaw: 3-16ths of an inch total—3-32 inch a side.

Mr. Parshall. Mr. H. F. Parshall: In the discussion it seems to have been assumed that in practice the length of the air gap is of primary importance in determining the qualities of a dynamo as to sparking, and changing of lead of the brushes. So far as projection armatures are concerned (and this is the class under discussion), I believe a more specific statement as to the influence of the length of the air gap is desirable.

The length of the air gap may be determined from the effect of unbalancing the electrical or magnetic circuits in the armature, due to the eccentricity of the armature with respect to the poles, which may occur in practice. In this case the length of the air gap may be considered as dependent upon conditions purely mechanical, and the length of the air gap would be the same for the particular type of armature under discussion as for any other armature of the same dimensions. The question becomes, then, whether or not, outside the limits of the length of the air gap fixed from mechanical considerations, a lesser air gap is permissible with the type of armature under discussion than with an ordinary type of projection armature. If it can be shown outside the limits fixed that the distortion is not dependent on the length of the air gap, and that the self-induction of an armature coil while in the arc of commutation does not vary greatly with the length of air gap, it follows, I think, that the influence of the length of the air gap on sparking is not of primary importance.

Referring first to the paper on dynamo-electric machinery by Dr. John Hopkinson (*Philosophical Transactions of the Royal*

Society, May 6th, 1886), under the effect of the current in the armature it was shown that the distortion, δ, or difference in magnetic intensity at the extremities of the pole arc, was equal to Mr. Parshall.

$$2\,K\,M\,C = l\,(a + \delta) - a) = l\,\delta,$$

where K is approximately the length of the pole arc, and M C is the value of the magnetising force of the armature, and l the length of the air gap. This expression, as was subsequently shown, is correct for smooth-core armatures. It requires modification, however, for projection armatures, in which case the magnetisation of the projections and the length thereof have to be taken into consideration. The expression for this is,

$$2\,K\,m\,c = l\,d + \left(a + \frac{d}{k}\right) f_1\,l_1 - (a)\,f_2\,l_1,$$

in which a and $\left(a + \frac{d}{k}\right)$ are the magnetisations in the projection at the extremities of the pole arc, k being a factor dependent on the ratio of the section of the pole to the section of the projections, l_1 the length of the projections, and f_1 and f_2 the magnetising force. It will be noted that, by magnetising the projections to a comparatively high value, the length of the air gap, l, becomes of minor importance, so far as distortion is concerned, since, if the magnetisation at the pole-face is initially high, any small shifting of the magnetism is accompanied by a very great increase of the magnetic reluctance in the projections, in which the density increases, this increase materially lessening the amount of distortion that would otherwise take place. I have found it possible in practice to limit the distortion to a very small amount by suitably proportioning the cross section of the projections to the cross section pole-piece, and that this can be done economically.

The next question for consideration is to what extent the self-induction of the armature coils is affected by varying the length of the air gap outside of the limits fixed from mechanical considerations. I may point out, in the case of projections within the extended top, as shown in the illustrations before you, the self-induction of an armature coil does not vary greatly, even with very small air gaps, with the position of the coil around the

Mr. Parshall. armature, and would, in fact, remain substantially the same if the pole-pieces were removed. I have repeatedly found this true in practice. In the case of projections with straight sides, the increase of inductance and the coils within the limits under discussion is not greatly lessened by any increase of length of the air gap. This statement is easily shown to be true, and I have found by measurement is correct, as calculations would indicate. I may point out that the counter E.M.F. of self-induction is equal to

$$2 \pi n L c^{10-9} = E s,$$

where n is the frequency of reversal, and L the coefficient of self-induction, and c current a coil when in the arc commutation. For sparkless commutation this equals

$$2 \pi n L c^{10-9} = 2 \pi n M^{10-8};$$

or $L c^{10-1} = M$, when there is but one turn in each coil, where M equals the number of lines cut by the coil when under commutation. This formula, together with the distribution curve, may be determined as above shown. The lead of the brushes for any load may be determined very approximately; also the proper width of collecting brushes. I would point out, as one of the meanings of the formula, that the greater the coefficient of self-induction of an armature coil the greater the amount of sparking for a given change of current in the armature; hence the advisability of shaping the projections so that the self-induction of each individual armature coil is the least possible amount. I have examined the figures given by Mr. Sayers, and find that the length of the air gap as given by him is precisely the same as I have decided to use in similar cases, and entirely with reference to the mechanical conditions, considering that the length of the air gap outside of this limit was of minor importance, provided the design was properly proportioned. With reference to the economy in the use of materials gained by the use of the invention of Mr. Sayers, I am in doubt, from the figures he quotes, whether there is any saving in material, since the machine as a whole does not seem to be any lighter than several good types of multipolar machines with which I am acquainted. Perhaps some further development of the invention may warrant the use of

this additional armature winding. In its present state, however, I do not see that the additional complication is warranted. Mr. Parshall

Mr. Esson.

Mr. W. B. Esson: This question of slotted armatures possesses a good deal of interest for me, because I used to make slotted armatures about ten years ago. But in my armatures, so far from the air gap being two-thirds of the width of the slots, as in Mr. Sayers's, it was only about one-third, and at that no trouble with sparking was experienced. We had none of the complications present in the machines to-night described, for, as a matter of fact, it was found that when the clearance was made sufficiently large to prevent heating of the pole-pieces the machines worked quite sparklessly. They ranged in size from 6 to 60 kilowatts, and from 10 to 800 volts, but the construction was given up simply because of the cost. Of course I look at this question entirely from a manufacturer's point of view, and, though I know of cases in which slotted armatures are preferable to smooth ones, for ordinary cases I think the latter have the advantage.

The question is, How does the cost of Mr. Sayers's machine compare with the cost of an ordinary one? and does the extra labour involved overbalance any gain due to saving in materials? The magnets of the 80-kilowatt machine are, I understand, of steel, and from a steel founder's point of view they are apparently rather complicated. They would probably cost 25s. per cwt.

Mr. Sayers: No.

Mr. Esson: And you can buy forgings tooled all over at 18s. per cwt., as many as you like. Another thing about that machine strikes me as disadvantageous. If you want to vary the size—perhaps make a machine of the same diameter, but six inches longer—the magnet patterns have to be altered, some carving has to be done; and every manufacturer knows what an expensive job pattern-making or pattern-altering is. If anything goes into the pattern shop, it seldom comes out under a few sovereigns. Again, it seems to me that the cost of jointing and connecting up the commutator coils and fitting the reversing poles add considerably to the cost of the machine. Also, with the number of slots varying for each winding, a manufacturer can scarcely stock dynamo carcasses, which may have to be wound for any speed, current, and E.M.F.

Mr. Esson. In 1885 I wrote for the *Electrical Review* some articles on this very subject of smooth *versus* slotted armatures, and was careful to remark "that, apart from mechanical advantages, the only "electrical advantages to be gained from the projections takes "the form of a diminished resistance of the air space for a "given area of polar surface." To this I still hold; it is simply a question of material saved on the one hand, against additional labour put into the machine on the other, and a point is reached when the slots are of no benefit. Apart from the simple question of smooth *versus* slotted armatures, it does seem to me that the saving consequent on strengthening the field by giving the brushes a trail instead of a lead, is not worth having at the cost of the complication Mr. Sayers introduces. I should like some definite information on this point of cost. It is in the labour we want to save. We can always put in an extra £10 worth of material to save £7 worth of booked labour and still be a little to the good. What does the extra labour cost? I should like to have the particulars from Mr. Sayers direct. It is perhaps too much to ask a manufacturer to display his prime cost book, but it is according to the opinions we form of the cost that this invention will be judged. I am quite willing to admit that, from a scientific point of view, Mr. Sayers's design constitutes a step of great interest in the construction of dynamos; but, like other speakers, I must ask for a little further information before I accept quite all that is claimed for this machine.

Mr. Addenbrooke. Mr. G. L. Addenbrooke: I should like to say a few words on this subject. I have taken some interest in the machine, particularly during the last six or eight months, and had the advantage of seeing, during the trial at Messrs. Belliss & Co.'s, the identical dynamo which Mr. Sayers has illustrated—the 80-kilowatt machine. I was present during a great part of the trial, and I have also since had the opportunity of seeing some of the machines under construction in Messrs. Mavor & Coulson's works.

The first thing I think we have to consider is that we have to deal with an accomplished fact. On the last occasion when this machine was brought before the Institution the whole was perhaps more or less of a speculation, but a large number of these ma-

chines have since been built, and are working very satisfactorily. The question is, What advantages have they over ordinary machines, from the user's point of view? Most of those who have spoken up to the present have been builders of dynamos; and everybody, of course, does not care to change their practice, and naturally think it is necessary to criticise details considerably. I represent, perhaps, rather a different point of view; and, looking at the machine as something that one might apply to do work, I must say I think that it is a very considerable advance on previous practice. Perhaps there may be something in what Mr. Ravenshaw shows us, but I think that something of that character must come in. In the first place, of course it is a very great advantage to have all the conductors positively driven. Then there is the other point that, having the conductors beneath the surface of the iron, we are not limited to the diameter of the armature. It is very easy to increase the diameter a little—the extra increase is not very much—and the consequence is, plenty of room can be taken in the slots for the conductors, and there is no reason to run them at a high density. These slots also give room for Mr. Sayers to put in his particular device. Then, of course, the size of the machine is certainly much reduced—much more than perhaps one would imagine—and seeing that 80-kilowatt machine at Belliss's in comparison with several machines of other makers of somewhat similar outputs, it seemed almost a toy; and there is no doubt that where weight and compactness are considerations the machine will be very desirable. Mr. Addenbrooke.

As regards keeping the brushes in an absolutely fixed position, it is only fair to say that I do not think that was quite attained. The arc through which the brushes required moving is certainly small; but they do require some movement, and I think it follows to a certain extent from the nature of the problem. We require to reverse the current in the coil under the brushes.

Now what we want to get rid of is, of course, the energy which exists in the coil before it is cut out of the circuit, and this energy is dependent upon the amount of current that was flowing in the coil. Consequently, when there is a great current flowing through the coil, we need to bring the line of commutation nearer the pole-tips and into the stronger field. Supposing that, instead

Mr. Addenbrooke. of putting the conductor outside, you put it in a slot, and at the same time bring the pole-tip very much closer to the surface of the core: you still get the same action, but through a very much smaller angle. Yet here, in order to get rid of the greater self-induction which is produced when a large current has been flowing through the coil, you must bring the coil, or Mr. Sayers's prolongation of it, a little nearer the pole-tip. Then there is another point to be considered in getting rid of the self-induction in these coils. You have to produce a back E.M.F. having the same curve as the E.M.F. in the coil—that is to say, its rate of change must be equivalent, as well as its magnitude. Consequently, for perfect correction you are dependent on the shape of your pole-piece and the exact nature of the lines at that point. In the case of the Sayers machine, where you have an armature with a lot of slots, the induction, as Mr. Swinburne has pointed out, jumps from one to the other. You are not dealing with a coil which is passing gradually into a field, as in the ordinary continuous-current machine. The lines that pass through the copper conductor are very small in number in comparison with what they would be if the conductor were situated outside; in fact, there is no doubt that the actions in continuous-current machines are very complicated. The more one looks into it, the more one sees that, in order to understand continuous-current machines thoroughly, one must have a most perfect knowledge of alternating machines; and when we have got that, and a perfect knowledge of two-phase machines, the question is whether two- and three-phase machines will not be better for most of the purposes for which the continuous-current machine is adopted.

Mr. Hawkins. Mr. C. C. Hawkins: Through the courtesy of Messrs. Mavor & Coulson, I also had the opportunity of seeing the 80-kilowatt dynamo described by Mr. Sayers running on full load, and I can bear witness to the efficacy of the auxiliary pole-pieces which are shown in Figs. 3 and 4; without them, I think it very doubtful whether 800 amperes could be successfully commutated in a dynamo with so short an air gap. The chief objection to their use is that, to an appreciable extent, they increase the magnetic leakage. From Fig. 5 it is easily seen that many of

the lines flowing through the reversing pole-pieces immediately return into the portions of the iron covered by the exciting coils. Mr. Hawkins.

There is one point which is perhaps worth mentioning in the dynamo invented by Mr. Sayers. In the ordinary continuous-current dynamo, commutation is effected by simply moving the brushes, and so moving the sections to be commutated into a sufficiently strong reversing field; but by the Sayers winding commutation is effected by means of the difference in the E.M.F.'s generated in two reverser bars. In fact, the differential effect is the essence of the whole device. Now, if the brushes are set so as to be sparkless at no load, and as the load is increased it is found necessary to shift them backwards, or towards the trailing pole-tips, it is by no means clear whether this adjustment of the brushes is necessary because the reversing E.M.F. is too great or too weak. The armature reaction as soon as the load is increased, tends to propel the fringe of lines outwards from the trailing pole-tips, and if this fringe of lines reaches the hindermost reversing bar, and does not reach the leading bar, the differential reversing effect is far in excess of what is required. This want of adjustment can be corrected by shifting the brushes backwards. To take an extreme case, if the brushes were shifted so far backwards that both the reversing bars came under the trailing pole-piece, the reversing E.M.F. would be very much less, and yet the casual observer, having in mind the ordinary dynamo, might suppose that the further the brushes are moved backwards the greater would be the reversing E.M.F. It may be argued that the same non-sparking position could be obtained by shifting the brushes forward; but, owing to the very rapid way in which the field falls off from the edge of the pole-pieces, it may not be possible to obtain a non-sparking position. Thus in the Sayers dynamo, especially with a non-reversible armature, it may be necessary to shift the brushes backwards in order to get less reversing E.M.F.

In one section of his paper Mr. Sayers foreshadows the use of a very small number of commutator segments, and on this score claims an advantage in the design of armatures for large dynamos carrying heavy current. I think that it will be found in practice

Mr. Hawkins. hardly possible to work with a number of segments much less than, say 30. The ordinary proportions of the armature for large currents would lead the designer to choose as the number of bars, say 60 to 72, and with the usual drum winding this gives a commutator of 30 or 36 segments. The next lower step must be 15 or 18—an extremely low number. Mr. Sayers has illustrated his point by starting with a 48-part commutator, and then lowering the number of parts to 24, which might be practically possible; but in many cases I think it would be found that the dynamo would ordinarily have, say 30 or 36 parts, and it would then not be possible to reduce these to 15 or 18.

The efficiency which Mr. Sayers mentions in his paper sounds high. I do not know whether there is any representative of Messrs. Belliss & Co. present, but I suppose that the efficiency of the engine might be put at about 91 or 91½ per cent., and this gives for the dynamo an efficiency of 95 in order to obtain 87 per cent. over all. But on going into the figures it does not appear that this efficiency is at all unreasonable. From the exciting power given in the paper we can deduce the loss of watts in the exciting coils as 750; the loss of watts over the resistance of the armature would, I should suppose, be about 1,600; and thus we are left with a margin of 2,000 watts, which probably might be divided into 1,000 for hysteresis and friction, and 1,000 for eddy-currents. In some experiments made by the firm with which I am connected, the power required to revolve a tunnel-wound armature in the excited field was measured, and thence was deduced the loss of power with the no-load distribution of the magnetic lines. From these experiments it appears that the eddy-current loss, whether in the bars, in the pole-tips, or in the discs themselves, is not very large. The only doubt that arises is as to how far these determinations can be really applied to the full-load distribution of the field, as there is no doubt that the massing of the lines under the trailing pole-tip and under each trailing corner of the divisions in the magnet, such as we see in Fig. 5, leads to a greater loss from eddy-currents and hysteresis at full load than at no load. In an ordinary continuous current dynamo, if the eddy-current loss is

measured in the way I have mentioned, the determination can be applied to the full-load case by increasing it by 25 per cent. at the most; but it would be interesting if anyone could give us information as to accurate tests on the efficiency of tunnel-wound armatures at full load. Mr. Hawkins.

The PRESIDENT announced that the scrutineers had reported the following candidates to have been duly elected:—

Foreign Member:

Julius Martin.

Associates:

Nigel Harington Balfour.
George Sydney Flood.
W. E. Groves.
Alfred Ingram.
Alfred Jacob.
Gerald Graham Palmer.
Walter Pethybridge.
Percival John Pringle.
Henry Walker.

Students:

Bruce Charles Bouquet.
Frederic William Close.
Walter James Coles.
Henry Thomas Constable.
Urban Baly Gilbert.
Ernest Herbert Lanham.
Charles Histerman Merz.
Henry Joseph Norton.
Harold Yarington Packard.
Archie Corbet Seaton.
Harold Langton Tyson-Wolff.
Frederick Morier Walker.
Frank Wilson.

The meeting then adjourned.

The Two Hundred and Seventy-fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 28th, 1895—Mr. R. E. Crompton, President, in the Chair.

The minutes of the Ordinary General Meetings held on the 14th and 21st of February were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Foreign Members to that of Members—

Robert Winthrop Blackwell.

From the class of Associates to that of Members—

Arthur Molyneux Sillar.

From the class of Students to that of Associates—

Percival Charles Austwick.	H. Sidney Smith.
Albert C. Ford.	Alan Smout.
Edward Stanley Franklin.	Struan W. G. Tamplin.
Alfred C. Ivimy.	Walter A. Vignoles.
Wyatt Meacher.	Ernest H. Walters.

Dr. Du Riche Preller and Mr. Barnett were appointed as scrutineers of the ballot.

Donations to the Library were announced as having been received since the last meeting from the American Institute of Electrical Engineers, Dr. Bedell, and Mr. T. L. Millar, Associate, to whom the thanks of the meeting were duly accorded.

The Chairman: We will now resume the discussion on Mr. Sayers's paper; but that gentleman informs me that he has a few words to say in amplification of his paper.

Professor S. P. Thompson: I take it that there is a main line of ideas running through Mr. Sayers's work, which diverges in two directions. He wants to get over the eternal difficulty of the sparking at the brushes, and, having got over that difficulty, to rearrange his machine so as to employ the iron and the copper to a greater specific advantage—that is to say, to gain from the manufacturer's point of view. Let me deal with the method of preventing sparking at the brushes by means of which Mr. Sayers has been enabled to use the armature to magnetise wholly or in part its own field magnets. What are the ways which have hitherto been suggested of preventing sparking? The original way, which we still use, was to give the brushes a forward lead, so that the reversal of the individual coils, or groups of coils, might take place just at the moment when they were entering a magnetic field of the right kind to balance their internal E.M.F. and to reverse the current in spite of the self-induction. But there are a number of auxiliary devices employed to enable sparkless reversal to take place without putting the brush into that forward position; and I take it that the reason why those other devices have been sought for is because, when you put forward the brush for the purpose of getting sparkless reversal, you, unfortunately, at the same time produce a tendency to demagnetise, and therefore prevent the machine from having the good regulating qualities which it should have. It has been proposed to employ an air blast to blow out the spark; it has been proposed to create, somewhere or other in the neighbourhood of the commutator, an auxiliary magnetic field. Both these methods assume that the spark is there to be blown out. Then there is the method of auxiliary poles, which we know has been attempted by many inventors, including Mr. Swinburne, and which was dealt with in the papers read four years ago before the Institution by Mr. Esson and Mr. Swinburne. I think Mr. Sayers, at an earlier period of his own development, proposed a method very similar to that of auxiliary poles, viz., by notching the pole-piece so as to obtain a sparkless reversal at some particular point of the revolution, instead of having to put the brush forward near to the tip of the forward pole-piece. Then there came the method

Professor Thompson.

Professor Thompson.

which is generally known by the name of Prof. Ryan, which was independently suggested also by Prof. Forbes and others at the same time. That method was winding a series coil over the armature—across it, in fact—so as to compensate the cross-magnetising effect. That method of cross-compounding by series conductors threaded in and out of the pole-pieces at right angles to the ordinary magnetic field going through the armature has been employed in America; but I do not know that any European manufacturer has resorted to it. It has two serious disadvantages: it complicates the construction somewhat, and it interferes to some extent with the ventilation and the accessibility of the armature. It is not so easy to make the armature easily accessible if you have these auxiliary coils crossing over the fairly wide gaps that there generally are between the tips of the pole-pieces on either side of the armature. Those being the older methods of fighting against the causes of sparking, there are the newer methods which Mr. Sayers introduced two years ago, and now in a perfected form with these regenerative armatures of his. The objection that was taken when he read his former paper—that his armature could only be used for revolving in one direction—he has now removed by the further development of bringing the commutator coils round at two places on the armature. If I understand his description aright, he employs that auxiliary pole-piece which we see indicated by the blue shading to cause his auxiliary coil—his compensating coil—to undergo an inductive effect after it has passed away from the influence of the pole-piece proper—an effect which reverses in that auxiliary coil the E.M.F. which the auxiliary coil received a moment before when the hinder side of it came out from under the pole-tip proper. That is to say, that he is using the two edges of his compensating coil —the hinder edge first, and then the forward edge afterwards—as it passes underneath that auxiliary pole.

Now I do not quite understand in what sense this armature is reversible. If I understand it rightly, the armature would work equally well if we were to take it out and put it into another machine which had the auxiliary pole-piece pointing the other way, in which case you would reverse the rotation from the left to

the right. But, if it is to be truly reversible and yet be a proper compensating or sparkless armature, you must not simply reverse its sense of rotation, but you must reverse also the direction of the auxiliary pole-piece. You will have to take these out and put them in the other way. [Mr. SAYERS: Certainly.] I notice, among other points, that the armatures proposed by Mr. Sayers differ from those which he described to us two years ago, in being all of the drum construction. I have not considered the matter, but perhaps he will tell us whether the new method is equally applicable to ring winding. In ring winding, where individual coils are threaded in and out of a ring, the adoption of this reversible construction would either require that each commutator coil should consist of two as ring windings, a little distance from one another on the ring, or else that they should be, as it were, an auxiliary drum winding on the outside of the ring. I should like to know whether Mr. Sayers has in fact made these ring armatures with reversible compensating winding. Professor Thompson.

Before passing away from this subject of the prevention of sparking, I should like to know whether any member has had any experience of two other ways of mitigating the difficulty of sparking at the commutator. Has anyone had any experience of running the whole commutator under oil? We know what valuable properties oil insulation has in preventing sparks. Has anyone tried running a commutator actually in oil, so that the spark may be crushed out of existence by the oil instead of being blown out or killed by a magnetic field? Secondly, has anyone had any experience of the method of avoiding sparks which has recently been described by Messrs. Hutin and Leblanc, in a machine which they have invented for the purpose of commutating alternating currents into continuous currents? I am not very well informed as to the work which Messrs. Hutin and Leblanc have done during the last year or two on this matter. They have published many short communications, and described a great many forms of machines, some of them alternate-current motors, some alternate-current transformers—that is to say, for transforming alternating currents of one kind into currents, whether alternating or polyphase or continuous, according to

Professor Thompson.

circumstances. In one of their recent papers they have described a method of damping out the spark which otherwise would occur in the use of revolving apparatus to commutate alternating currents into continuous currents. The commutating of alternating currents into continuous currents is so old a theme that one would think that there is nothing new to be said about it. But it is clearly most desirable that the electrical engineer under present circumstances, where alternating-current distributions are so widely prevalent, should have a thoroughly practical means of tapping that alternating current and commutate a portion of it into continuous current for electrolytic and metallurgical purposes. Not until something of that kind is done shall we be entirely satisfied to see the whole of electric supply carried on by means of alternating currents, which otherwise have, as we know, so many advantages. The commutating apparatus of Messrs. Hutin and Leblanc has one peculiarity of construction which on paper looks very nice indeed, but of which I have had no experience. I want to know if anyone else has had any experience of its operations. I will briefly describe it. We know when an armature is being used in the ordinary way, revolving between pole-pieces, that a certain portion of the coils—viz., that which is included in one section from one bar of the commutator to the next—has to have its current reversed in it while it passes, so to speak, "under the brush." That operation occupies a little time, and during that time this section moves forward through a small angle—certainly not through its own breadth always;—the reversal may take place much quicker than the time occupied by the coil in passing through its own breadth. The consequence is that a certain amount of winding, with the current in it, is virtually transferred from belonging to one half of the coils to the other half of the coils, and the magnetising action of the coils in the two halves of the armature conspires to produce a resultant pole, or field, which is shifted during the operation of reversal. The pole is at a certain instant at one end of the advancing section about to be commutated. A short time afterwards it is shifted on to the other end of the section, as though the magnetic field due to the coil jumped back from one

Professor Thompson.

side of it to the other. But during that time the coil has moved a little bit onwards, so that the actual angular jump of the magnetic field due to the coil is not so wide an angle as the breadth of the section itself. At any rate, the effect is the same as though at the commutation of every coil there was a small jump, or vibration, backward and forward of the magnetic field of the armature. Messrs. Hutin and Leblanc enclose the entire armature in a stationary squirrel cage of copper; that is to say, they perforate the pole-pieces just below their surface with a number of holes all round. I do not know how far this is carried out beyond, but it may be carried through the whole periphery. A number of parallel copper bars are inserted, and they are all short-circuited right round by a copper band in front and behind. The armature runs within a squirrel cage embedded in the pole-faces. So any tendency to shift the magnetic field backwards and forwards will certainly set up eddy-currents in this squirrel cage. The currents tend to flow up one bar and down another bar, and always in such directions as will tend to oppose any change in the magnetic field. The result, I am told—and it ought to be so on paper—is that it maintains the magnetic field in a uniform position, in spite of this commutation taking place. Therefore you may fix your brushes once for all and have no sparking. If all this is realised I think it is an extremely important thing. Although they have not described it as a means of stopping the sparking of an ordinary dynamo, they have described it as a means of stopping sparking in what is really a much more difficult case, viz., in the transforming of alternating currents into continuous currents; and if it will prove effective in one case, it will do so in the other.

Returning to Mr. Sayers's paper, I should like to ask him why he mixed up together the terms "slotted" and "tunnel" as applied to armatures. To my mind they mean quite different things. The slotted armature is nothing more than our old friend the toothed armature of Pacinotti, concerning which much has been said and written in former years, and which needs no further advocacy from me. The tunnel armature I take to be a construction which was used years ago by Mr. Swinburne in the construction of certain machines with a small air gap, and which

Professor Thompson.

in modern times has been associated mainly with machines made in Switzerland by the Œrlikon Company and by Mr. Brown. The distinction is so clear, as a matter of construction, between having holes clean through the armature and having slots between the teeth at the periphery of the iron, that I think it is a pity to mix up the two terms together. The difficulty urged against the adoption of slotted armatures—viz., that insulation becomes more difficult—is one that has been got over in a number of different ways. Mr. Sayers does not say precisely how he gets good insulation in these slots, and perhaps he would not mind telling us. In 1893, in looking at the machines, not only in the Chicago Exhibition, but in various factories up and down the United States, I was very much struck with the fact that so very small a proportion of the armatures of the machines, whether continuous-current, alternating-current, or motors, had any smooth armatures: the smooth-core armature, in fact, appeared to be an entirely obsolete construction in America. Of the hundreds, if not thousands, of armatures I looked at during my hurried visit to the States, certainly not 5 per cent. were anything except slotted armatures. The smooth armature was regarded as entirely a bygone construction. It was to me interesting to see the discussion raised here again a fortnight ago as to whether the smooth armature was not possibly better than the slotted one.

I should like to ask Mr. Sayers another question. He remarked that he had not to alter the lead *much* on these machines when the load increased. When machines are made in this way with the regenerative armatures that enable him to put the brushes backwards instead of forwards, what has to be done to the lead of the brushes as the load goes on? Do you have to put them forward or backward? [Mr. Sayers: Backward.] I suppose the thing will really depend to what degree the compensation is carried out.

Another point I should like to ask for further information upon is, in what cases does it occur that you want to run the machine both ways? Is there any case, except train-lighting, where you run your dynamo off the shaft of a carriage that has to go some-

times forward and sometimes backward? is there any other case where it is really important to have an armature which will run in every direction? It seems to me that there are not very many. The difficulty of having to take off the brushes and reverse their direction, if you use the machine for any other purpose, seems to be not a great one; not that I would underestimate the value of an armature that would run both ways, but it seems to be hardly worth while, seeing that it is very seldom you require for dynamos to have a machine running both ways. With reference to the formulæ which Mr. Sayers gave us on the last occasion, I would remark that the "machine constant" seems to have a physical meaning. What is it except something proportional to the torque of the machine? The torque multiplied by the speed gives you the output of the machine if you take the proper units. Therefore the output of the machine, divided by the speed, will give you the torque. It is, too, quite natural that the square root of the torque should be proportional to the magnetic field at constant speed; for if you drive your machine at constant speed the field will be proportional to the E.M.F., and the current will be proportional to the E.M.F. Therefore the product of the current and the E.M.F. at the output will be proportional to the square of the field; so that, the speed being constant, we ought to have the second relation, as a matter of first principles, that the field of the machine is proportional to the square root of the torque. I was hoping that Mr. Sayers would have said something about the use of his machines as continuous-current transformers. We know that he has done some work in that respect, using some of these short-air-space compensated armatures for the special purpose of giving the extra E.M.F. that is wanted for distant transmission of the current, or, to use the expressive American term for such machines, used as "boosters." May we know something more about these and their performances? Professor Thompson.

Mr. Sayers gave us some figures about the weight of this 80-kilowatt output machine. Certainly the figure obtained is fairly high. I have been thinking rather of the matter from the point of view of the motor. We have a machine here which weighs 67 lbs. for every H.P. which it exerts, if it is run as a motor.

Professor Thompson. When in 1883 Professor Ayrton read a paper about motors, in which he laid so much stress upon the weight of a motor, trying to find a construction which would have the lightest weight for a given power, the best figure he could arrive at was 90 lbs. cross weight per H.P. We have gone on a good deal during the years that have elapsed since 1883 in various matters of motor construction; but is not this rather a heavy weight still? Is it not true that if you adopt one of the modern polyphase methods of transmitting power, your motor, if you get up to anything like large powers such as 80 kilowatts, will not weigh more than half this? There seems to me to be some discrepancy here. I think we ought to be able to make a continuous-current machine (though I do not say it would be always advantageous) which would have less weight per H.P. than 67 lbs. In the abstract there is a way of looking at the use of copper and iron, which ought to throw some light upon the question of the construction at minimum weight. There are reasons for arguing that the specific output will be greatest, for a given total heat waste, if the copper on the field magnets and on the armature are of equal weights.

Is it advisable to go so far as Mr. Sayers seems inclined, and put all the copper into the armature and leave none on the field magnet? I think it will be found that it is not advisable to carry the construction too far in that way. You will not get a minimum weight of copper, and therefore a minimum cost, as far as materials are concerned, by going too far in the method of using the armature to magnetise the field magnets. I do not, however, wish to express a too strong opinion upon this point.

Let me conclude by repeating my congratulations. There is hardly anybody at the present moment working now at the old common-place problem of improving continuous-current dynamos; the one man who has done anything in that way during the last two years is Mr. Sayers, and we have to thank him for having brought his work here.

Dr. Preller. Dr. Du Riche Preller: The point to which I wish more especially to refer, in connection with Mr. Sayers's paper, is the considerable saving in weight, and also, I take it, in cost, which he

claims for his reversible and regenerative dynamos with slotted armatures. The type of direct-current dynamo and motor with slotted armature with which I am more particularly familiar is that of C. E. L. Brown; and it may not be uninteresting if I make a few comparisons between Mr. Sayers's reversible and regenerative machine and that of Mr. Brown's ordinary standard type. In the first place, I should like to point out that mere reduction in weight does not necessarily mean reduction in cost; and although, of course, it is very easy to reduce the weight of a motor or a dynamo, it is not advisable to do so beyond certain limits, more especially if the machine is intended for normal work. Now, even granting that Mr. Sayers has reduced the weight of his machine, I do not think it shows any gain whatsoever over such machines as those of Mr. C. E. L. Brown. For instance, the weight of Mr. Sayers's 80-kilowatt, or 100-H.P., machine—exclusive, however, of the bed-plate and bearing—works out at 3·4 tons, or 92 lbs. per kilowatt. Mr. Brown's 60-kilowatt (80-H.P.) four-pole steel-clad motor, 450 revolutions, for high-speed traction, only weighs, *complete*, 2·7 tons, or 100 lbs. per kilowatt; and, again, his 90-kilowatt (125-H.P.) motor, 450 revolutions, weighs 3·3 tons, or 100 lbs. per kilowatt. The ordinary type of Mr. Brown's direct-current 80 kilowatt (100-H.P.) generator is a four-pole machine making 420 revolutions, and weighs 4·5 tons, or 126 lbs. per kilowatt, including the bed-plate and its two bearings, which weigh 1·2 tons; exclusive of these, the weight of the machine is therefore 92 lbs. per kilowatt, or the same as Mr. Sayers's dynamo. Even this weight could easily be further reduced, but I know Mr. Brown very wisely refrains from doing so for normal ordinary work. Of Mr. Brown's two-pole type, I may mention his 110-kilowatt, or 150-H.P., high-tension direct-current dynamos and motors (4,000 volts, 550 and 450 revolutions respectively), such as are used in a long-distance power transmission for a cotton mill in Italy. These machines weigh 5 tons *complete*, or about 100 lbs. per kilowatt. All the machines I have mentioned have air spaces of 8 mm., or one-third of an inch, and, as I can attest from numerous instances within my own knowledge, run absolutely without sparking. With regard to the question of Mr. Sayers's short air space, I

Dr. Preller.

Dr. Preller. think Mr. Parshall has already disposed of that question, so that I need not refer to it again.

The next point I should like to mention is that of the dynamo efficiency. I think Mr. Sayers, in his supplementary remarks, gave the efficiency of his machine as 95 or 96 per cent. That may be a test efficiency; but I am disposed to doubt whether it gives that efficiency in actual ordinary work under varying conditions. Mr. Brown only claims an efficiency of 93 per cent., the armature copper loss being about 2 per cent., and the shunt loss about the same. But this efficiency, be it noted, is attained under all conditions of working, and, moreover, includes the losses due to ventilation, and to friction in the bearings produced by the weight of the armature and, in case of indirect driving, by the belt tension. These losses represent at least 2 to 3 per cent.; so that, if these be disregarded, as seems to be the case in Mr. Sayers's efficiency, the dynamo efficiency of Mr. Brown's machines may easily, and very properly, be stated as 95 per cent.—viz., quite as high as that claimed by Mr. Sayers. Higher efficiencies on the same basis are, as is well known, only to be attained by very much larger machines.

As regards the question of cost, the weights given by Mr. Sayers for his machine warrant the conclusion that the cost of his machine would come out higher than that of either the bipole or four-pole Brown machine of equal output. Including the bed-plate and bearings, the weights of which it is desirable that Mr. Sayers should give, his machine would, according to my calculation, probably cost at least 15 per cent. more than Mr. Brown's type.

As regards the name of "reversible" and "regenerative" machines, I agree with Mr. Mordey that it would be much better to call the machine simply "Sayers's dynamo." As regards the terms "slot" and "tunnel," I own that I was much surprised when I saw the heading of the fourth section in Mr. Sayers's paper, "Slotted, or Tunnel, Armatures." As Professor Thompson has already pointed out, the two things are entirely different and distinct; for a slot, being an open groove, is one thing, and a tunnel, being a hole or perfora-

tion, is another. For instance, the holes in Messrs. Hutin and Leblanc's device, or the peripheral perforations in Brown's alternate-current motors for inserting copper bars or windings, are tunnels; whereas Mr. Sayers's arrangement is a slot pure and simple. In the heading of his fourth paragraph, Mr. Sayers has put a comma between the two words—"Slotted, or "Tunnel, Armatures." I do not know if he intends the two terms to be synonymous or distinct from each other; and it is important that he should make this point clear, as the promiscuous use of the two terms is misleading. In conclusion, I may say that I fail to detect any great practical advantage in Mr. Sayers's form of dynamo. The ordinary Brown machines with slotted armatures, to which I have referred, give the greatest possible satisfaction, both as generators and motors, for normal as well as for special work, without any such reversible and regenerative devices as those designed by Mr. Sayers. If by these devices Mr. Sayers has really succeeded in reducing the weight of his machine, all I can say is that it must have been too heavy before. Mr. Preller

Mr. S. Evershed: I hope I may be forgiven if I remind the meeting that the subject under discussion is not the definition of the words "tunnel" and "slot," but, rather, given an armature with slots or tunnels, how are you going to commutate it sparklessly? I think it would be rather unfortunate if this discussion were to be left in the position in which Professor Thompson left it, as though the question of commutating sparklessly were one simply of getting rid of the spark, as though a spark were an unpleasant thing to look at. That is not really the question at all. The real problem is how to remove a coil from one circuit with its current in one direction, and place it in another circuit with its current in the opposite direction; and to do that without any damage to the commutator it is necessary to deal with the energy in the coil much in the same way as the energy in the piston and other reciprocating parts of a steam engine is dealt with by cushioning, as Mr. Sayers has very happily remarked. In reality, the question of sparkless commutation is one which is fairly easy to understand. You have, as I said just now, to take the coil out Mr. Evershed.

Mr. Evershed.

of one circuit and put it into another, and the first thing you have to do is to short-circuit the coil. There is no particular difficulty about that. Having got it short-circuited, you find some induction threading the coil; and the next thing you have to do is to get that induction out, and the best way of getting it out is to shove some through the coil in the other direction. This rapidly reduces the current to zero. Continuing the introduction of induction, the current immediately begins rising up in the other direction, and at a certain instant its value is exactly equal to that of the circuit into which you are going to put the coil—exactly of the right strength to be put into the other circuit. If you can happen to break the short-circuit at that instant, then you get sparkless commutation. Now where are you to get that induction from which you use in carrying out this reversing operation—what I may call the "negative induction"—if the induction due to the current itself is originally positive? There are practically three ways of getting it. The old way is to hunt for a place on the dynamo where there is some induction of the right sort, and the attendant hunts for it by shifting the brushes forward until he finds it; but that is obviously a very clumsy way of doing it. Then in 1887 (I think it was) Mr. Swinburne proposed—I do not know whether he was the originator of the idea; I hardly think he was—but, at all events, he proposed to carry out the reversing operation by means of reversing pole-pieces; and down at Chelmsford I had the pleasure of seeing a great many of his drawings and designs, and it appeared to me he intended to patent every conceivable form of reversing pole-piece. Mr. Swinburne, therefore, by means of reversing pole-pieces, was proposing to bring some induction to the place where the coils should be reversed. Mr. Sayers, as we see, prefers to take his short-circuited coil (or, to be exact, a convenient part of the "short"-circuit) to the induction; and because he does not want to have any back ampere-turns on his armature, as he would do if he shifted the brushes forward, he takes the connector between the coil and the commutator sector to a place under the flux horn where there is induction of the right value, viz., an induction

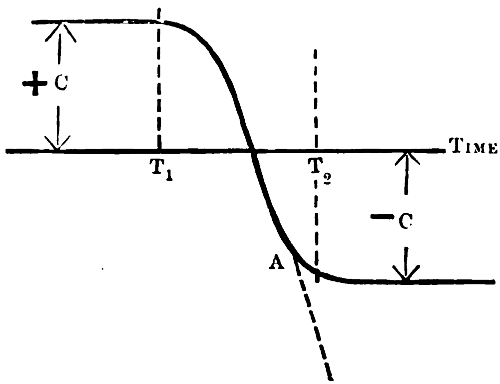
which varies in proportion to the current in the armature. That seems to me, on the face of it, a more complicated way of effecting the object in view than using a reversing pole-piece wound with a few main current-turns; because, in the first place, although the weight of the machine may be reduced, it is clear that the labour of winding the armature is increased. I think that goes without saying. The chance of breakdown is also considerably increased. But, as I had no opportunity of discussing this point when Mr. Sayers read his original paper on the question of commutator coils, it is perhaps hardly fair to discuss it now when he has made over 100 machines which are all working successfully—I have not the least doubt they are. It is a most ingenious way of reversing the coils, although, to my thinking, somewhat too complicated. Mr. Evershed.

But suppose this question of sparkless commutation has in one of these ways been got over, we are still some distance from the ideal notched armature, which should run with practically only the necessary mechanical clearance between its periphery and the pole-pieces; for, as we see from the diagrams on the wall —particularly Diagram 5—and the figures given in the paper, Mr. Sayers is obliged to have a comparatively large clearance in order to avoid eddy-currents in the pole-pieces. And the reason is simple. If you could wind the coils through holes in the armature—what some people call "tunnels;" I should think they might be called "holes"—you have such an enormous induction round them that the reversing pole-piece or commutator coil has greater work to do; and it is practically out of the question altogether in a large machine to laminate the pole-piece, so that really the first thing the designers have to do is to get over that difficulty. They must make the surface of their armatures smooth some way or other by means of iron, and they must make the best job they can of the increased self-induction.

To return to the reversing pole-piece (and also to the commutator coil, for my suggestion will refer to both), I said just now, when you have an armature coil under the brush, and therefore short-circuited, there is an instant when the current in it is of the right value to enable you to put the coil into the main circuit; but in most machines with notched armatures it is only

Mr. Evershed.

at the right value for an instant; and that is, or may be, a serious disadvantage in commutation, because, unless all the coils are absolutely identical in the value of their coefficient of self-induction, the reversing, or negative, induction that suits one coil will not suit another: you will not get the armature exactly sparkless for every coil. The question is, Can that be got over? and, if so, how? What we really have to aim at is, not only to make an electro-motive force in the coil by shoving negative induction into it, but after having reduced the current to zero and raised it to the right value again, we want to keep it there for a sensible time.



The curve in the diagram represents the current in an armature coil before, during, and after reversal; the normal current being + C, at time T_1, the coil is short-circuited and the current begins dropping, and you can make it drop as fast as you like by putting in the reversing induction at any rate you like. The current reaches zero, and, becoming negative, rapidly rises to the value — C at time T_2, and at that instant the coil must be put into the main circuit; and it is perfectly obvious, if the current is rising at anything like the rate indicated by the dotted line, it is next door to impossible to hit on the right spot for every coil in the armature. What you want to do is to arrange the reversing pole-piece, or the induction-cutting commutator coil in Mr. Sayers's dynamo, so that the current, having reached the value — C, remains there for a sensible time. When I say "a sensible "time," I mean sensible compared with the whole time the coil is

short-circuited. It ought not really to be a problem impossible of solution to arrange the introduction of the reversing induction in such a fashion that it shall be powerless to get the current beyond that value. You will then have a reasonable margin of time during which the coils can be put into the main circuit equally sparklessly, no matter if there is some small difference in their coefficient of self-induction. Precisely how that is to be arrived at it is not for me to say; but it is simply a question of introducing the reversing induction at a uniform rate, so that the reversing E.M.F. is constant during the required space of time. With reference to reversing pole-pieces, one method of attaching them to a Manchester type dynamo has been shown by Mr. Ravenshaw; but, although that machine ran absolutely sparklessly, that type of reversing pole-pieces has the serious drawback that the current-turns you have to put on them are approximately equal to the cross ampere-turns due to the current in the armature. If, however, you fit a reversing pole-piece between the other two poles, and give it an independent little magnetic circuit through itself and a small portion of the armature core round the coil under the brush, it would require very few ampere-turns on it to produce the right reversing induction, for the simple reason that you have not to fight against the cross ampere-turns in the armature. I cannot help thinking that some form of reversing pole-piece of that kind would be far more easy and far cheaper to deal with than the commutator device of Mr. Sayers. But, although that is my opinion, I must congratulate Mr. Sayers on being the only member of this Institution, as far as I know, who has had the courage to deal with this problem. We have had reversing pole-pieces, and the question of sparkless commutation, before us for 10 or 12 years, and no one has had the courage to tackle it, and effect a successful sparkless commutation, until Mr. Sayers took the matter in hand some two or three years ago. Mr. Evershed

Dr. Du Riche Preller: May I add one word with reference to a remark made by Professor Thompson—namely, that the weight of alternating-current motors was 25 lbs. per H.P. With all deference to him, I think that is too low. The lightest 100-H.P. alternating-current motor that I know is 2·3 tons, or 50 lbs. Dr. Preller.

Dr. Preller. per H.P., which is exactly double the weight mentioned by Professor Thompson; but even that is only two-thirds of the weight of Mr. Sayers's direct-current machine (100 lbs. per kilowatt, or 75 lbs. per H.P.), and therefore justifies Professor Thompson's contention that alternate-current machines are preferable.

Professor THOMPSON: I have not studied the question recently. I was speaking purely from memory.

Mr. Snell Mr. ALBION T. SNELL [*communicated*]: I was prevented from attending the discussion of Mr. W. B. Sayers's paper entitled, "Regenerative Armatures." I must frankly admit I do not understand the term as applied to armatures. It appears to me that "Sayers" armature would be more explicit and correct.

The paper is interesting as recording the building of an 80-kilowatt dynamo with commutator coils, and also because of the publication of Mr. Sayers's device of duplicating the commutator coils, by which it is possible to make reversible motors with these armatures. However, since they run with a forward lead, it is necessary to automatically adjust the lead as the motor is reversed. The arrangement proposed is similar to one tried by me five years ago for tram-car motors, and found then to be impracticable, from the difficulty of maintaining good contact between the brushes and the commutator. Mr. Sayers may be more successful in his mechanical details, and certainly his device prevents sparking at the moment of shifting the brushes.

I must confess, however, to some fear that the cost of these motors must be high in comparison with that of ordinary machines having wide brushes placed on the neutral line, as are used most successfully for a variety of purposes all over the world. I grant that by Mr. Sayers's device the lightest weight and best regulating qualities should be attained, but he must pardon me if at present I question the cost.

This, after all, is a matter which primarily concerns the manufacturers, and it will be solved in the ordinary course of affairs.

The President. The PRESIDENT: I must close the discussion. In summing up, I think there has been rather a tendency to undervalue Mr. Sayers's invention, not as an invention, but on the ground that

the plain slotted armature was sufficient. I think it shows want of knowledge on this subject on the part of those who made this remark. It is well known that the slotted armature has been gradually making its way from year to year, and that it presents advantages in the way of making a good stiff, strong mechanical job, and in reducing the quantity of copper required on the field magnets; but it always has been attended with disadvantages in the way of producing difficulties at the commutator, and Mr. Sayers is really the first person who has successfully tackled this difficulty. I wish to join issue with Dr. Preller when he compares the efficiency of Mr. Sayers's machine with that of Mr. Brown's. There is no man who has a higher respect for Mr. Brown than I have, but he designs machines for a market which does not demand such high efficiencies as are asked for in England. Mr. Sayers has been trying to produce a machine to compete with those of Siemens's Electrical Construction Company and others, my own firm amongst them, all well known to have an efficiency of 96 per cent., and sometimes 97 per cent. Such efficiencies are not asked for, and are practically not obtained, on the Continent of Europe. No doubt, if Mr. Brown were called upon to design such machines, he would produce heavier ones than those he now makes. On this side things are different. We have been educated up to the point of supplying these high-efficiency machines, and they are necessarily, as long as they are smooth armatures, somewhat heavy. The smooth-armature continuous-current machine of which Professor Thompson spoke rather slightingly is certainly one of the most perfect pieces of mechanism that the world has ever seen, but it is costly; and it is with the view of reducing this cost, and at same time to do away with the disadvantage of trouble at the commutator, that Mr. Sayers has brought forward his invention. I thought this was so well known that it would not be necessary for me to make these remarks; but it appears, from the tone of the discussion, that it is not so generally understood as it ought to be. The President.

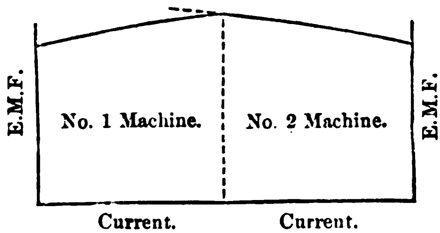
Mr. Sayers (in reply): I hardly know how I can do justice to the discussion in the time at my disposal. My first duty is to acknowledge the very kind expressions with regard to myself Mr. Sayers.

Mr. Sayers. which have fallen from the lips of several speakers, especially from our honoured President and Dr. Thompson. Amid this struggle for existence it gives one the keenest pleasure, and an altogether higher and brighter view of human nature, to find one's work appreciated in such terms as have been used towards myself. I feel far less entitled to them than are the speakers themselves in respect of their own labour and achievements.

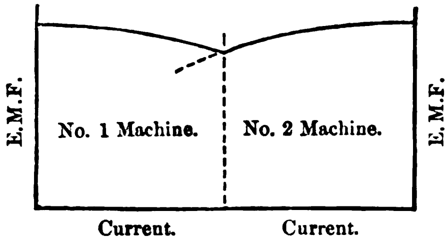
Mr. Andersen said that one of the objections to the use of the slotted armatures was the heating of the pole-piece. Now there is no heating whatever of the pole-pieces, and no measurable loss of power from that cause, if the proportions which I have given are adopted. Objections have been raised to my synonomous use of the words "slotted" and "tunnel." The reason is that in practice they merge one into the other. The crown is always cut through, but if the opening is very narrow the word "tunnel" expresses what is meant. There comes a point, however, when the opening is wider, when "tunnel" is inapplicable and the word "slot" is more suitable. I think, however, the case would be well met by the adoption of the expression used by Mr. Parshall for armatures of the kind under discussion, *i.e.*, "projection" armatures.

Then Mr. Andersen also made some remarks about running these machines in parallel. I thought it was pretty clear from the paper that the arrangement suggested in Fig. 8 is only for machines with a rising characteristic, and I do not think there can be many members who do not realise that; but I may just say that of course the coupling of continuous-current machines in parallel does not depend in the slightest degree on whether they have compound coils or forward coils on them: it depends upon the characteristic. The two diagrams below will make this clear. The first diagram represents the characteristics of two similar machines facing opposite directions. The machines are supposed to be coupled in parallel; and if they were giving equal currents, then the division between the two machines would come in the centre, as indicated by the dotted line. Now, if the machines have a rising characteristic, it is quite easy to see that they cannot possibly run in parallel; because, supposing No. 2 machine

is giving a little more current to the circuit than No. 1, this increase of current raises its E.M.F. so that it tends to give still more, while the increase in No. 2 is accompanied by a corre- Mr. Sayers.



Rising characteristic.



Falling characteristic.

sponding diminution in the case of No. 1: the inequality therefore rapidly increases, the conditions are unstable, and the arrangement will go slap over, No. 2 taking all the load and sending a back current through No. 1.

But if the machines have a falling characteristic, the result of coupling in parallel is quite different, and the arrangement is quite stable. The lower diagram represents two machines with a falling characteristic. If No. 2 machine gives too much current its E.M.F. tends to fall, while that of No. 1 rises, thus stopping the tendency at once, and the division of current between the two machines tends to become equalised—*i.e.*, the condition is stable. As stated in the paper, and by Mr. Mavor in the discussion, machines which have a slightly rising characteristic may be safely coupled in parallel when driven from independent sources, provided that increase of load reduces the speed sufficiently to make the characteristic of the combination a falling one.

Referring to Mr. Andersen's suggestion that in order to avoid risk of reversal it would be necessary to make the shunt more

Mr. Sayers. powerful than would otherwise be required, I may say that even with machines having the shortest air gap and least possible shunt winding the exciting turns on the armature would require a far greater current through them than could occur in practice, in order to reverse the polarity. There is, in fact, no risk whatever of reversal with the proportions I adopt.

Mr. Andersen was the first member who discussed the weight question. He gave the figure for a machine on the ordinary lines as 85 cwt., against my 64½ cwt. Mr. Parshall stated that his machines had about the same weight, and Dr. Preller has given us weights some of which are a little less than mine. The ratio I have found between my weights and those of other makers on the ordinary lines, comparing two-pole machines, is about 5 to 8 —5 for my machines, against 8 for other people's. Now, referring to the weights given by Mr. Parshall, and also those given by Dr. Preller, the point to note is that both these machines are four-pole. If I make my machine four-pole, I will make it half the weight.

Dr. Preller: They are not all four-pole. Here is a magnificent 150-H.P. motor with 4,000 volts, which is two-pole. It is about 90 lbs. or 85 lbs. per kilowatt.

Mr. Sayers: I think it is a matter of general knowledge that a four-pole machine is approximately half the weight of a two-pole. ("No.")* I think so, with the same output and speed. Considerably less, at any rate. I think no one will dispute the fact that a four-pole machine, output for output, and speed for speed, is a lighter machine than a two-pole one.

Mr. Mordey objects to the word "keepers." As I mentioned in the paper, I was not the originator of the expression; but I do not think that Mr. Mordey's criticism was altogether just: we are not so rigid in our nomenclature as all that. The alternative word for the thing referred to appears to me to be "yoke," but it has not got the characteristics of a yoke as the piece on a two-pole machine has. The armature does its own exciting; the armature is its own field magnet, providing we call the magnet

* I accept the correction; I had not given much attention to multipolar machines, and was under a wrong impression.—W. B. S.

the part on which the exciting coils are wound. The other part, Mr. Sayers. which is called the "keeper" by Lord Kelvin, is merely the piece of iron which completes the magnetic circuit, just in the same kind of way as a piece of iron put on to the old horse-shoe magnets completed the circuit. I think there is a similarity in the two things. It is a piece of inert iron which merely acts as a completion of the path. Then Mr. Mordey calls attention to the fact that I make my armature reversible by doubling the commutator coil. That is the case from a certain point of view, but from another point of view it is not at all the case. The doubling of the commutator coil—the making it reversible—allows of the use of the pole-piece P, one of my forms of polar projection. The use of that pole-piece is to increase the effectiveness of the commutator coil. The commutator coil is not twice as effective with this form of pole-piece as it is without, for the reason that the leakage which occurs from its general surface to the armature is considerable, so that the main coil, which is subject to the reversing force, is not working in anything like so neutral a field as it would be if the pole-piece were absent; consequently the effect of that pole-piece is not to double the effectiveness of the coils, but I think it increases them by about 50 per cent., or something like that.

Mr. MORDEY: If Mr. Sayers would make it clear to some of us why he uses a reversing pole at all, it would be of great assistance. Seeing that he has got the whole armature, and can range over it where he likes, and choose what E.M.F. he likes to get by his commutator coil, it seems to me the reversing pole-piece, if I understand it properly, ought not to be a needed condition.

Mr. SAYERS: I am just coming to the reason for using the reversing pole-piece. In the 80-kilowatt machine I have not taken full advantage of what I am going to describe, but there is undoubtedly in the use of that reversing pole-piece a means of very largely increasing yet further the output for a given weight of machine, for the simple reason that the amount of current which can be commutated is increased by its use to about 60 per cent. In this particular machine, to say the least of it, I know quite well that by simply increasing the weight of copper on the

Mr. Sayers. armature, and not making any other increase except in the necessary depth of tooth, and so on,—by simply increasing that weight of copper I can increase the output of that machine to 120 kilowatts. The reason is (and this deals with another point in the general criticism) that an increased load does not destroy my reversing field. I want to lay great emphasis on that. Several speakers seem to have had the impression that my main advantage has been to get the regenerative action from the armature with a view to keeping up the voltage, to compounding, keeping a level characteristic; but the chief advantage of the thing is that you cannot put enough current on to the armature to destroy the reversing field: you continually strengthen it by increasing load; you may get up to the highest possible saturation in the projections, it is true, and then you cannot get any further. Yet you have nothing like the conditions which you have in an ordinary machine, where, if your load exceeds a certain amount, you destroy your reversing field and have nothing to work with. Having got a reversing field, indestructible, as it were, by the current in the external circuit, the output of the machine does not depend in any way upon the total induction or upon the mass of iron, and is not limited by the cross ampere-turns. I know quite well, of course, that for economic reasons there will be certain proportions which will turn out to be best; but otherwise there is no absolute limitation with regard to the output; it is all a question of design. The reason for that is this: The self-induction of the armature conductors placed in slots, or tunnels, is, I take it, proportional to the length, the self-induction of the end connections being comparatively negligible. If you shorten an armature you reduce the self-induction in proportion; but if while you shorten the armature you increase the diameter, so as to keep N constant, then while you lessen the self-induction you do not lessen the reversing E.M.F. With my arrangement you actually increase it, owing to the greater distance which you get between your two commutator coils. The consequence is, that if you go on shortening length and increasing diameter there is no limit whatever to the amount of current you can commutate with a given amount of field. It is from

such considerations as these, and also the possibilities of what can be done with multipolar machines, that I make the statement that this machine is beginning, as regards weight and output, where other types leave off; and I am quite sure that it can go down a very long way with regard to weight. This machine is the first of its type, a very safe machine, and it is not pared down in any way; and I think the discussion has shown that, compared with machines of like efficiency, its weight is much less than either two- or four-pole machines of the ordinary type. Mr. Sayers.

Referring to Dr. Preller's remarks on the efficiency of the 80-kilowatt machine, the figure given includes all losses, and is the true commercial efficiency of the combined plant, and, as stated by Mr. Hawkins, means not less than 95 per cent. for the dynamo, including bearing friction, air friction, &c., as enumerated by Dr. Preller.

The President's remarks on the effect of the extra 2 or 3 per cent. in efficiency on the weight, make it unnecessary for me to add anything on that point.

Mr. Mordey was wishing to introduce innovations in spite of his objections to mine; he wanted to call this a "correcting coil" instead of a "commutating coil." I think the verb "to "commutate" means exactly what we are discussing, but I admit I have always found the expression "commutator coil" a cumbersome one.

Mr. Ravenshaw spoke of what he calls a "cross-field" machine, of which he has made an interesting diagram. His remarks were in the direction of showing that this was not the impossible construction which it was supposed to be; but I think, if Mr. Ravenshaw will attempt to work out a machine of this size, that he will find, although it may not be absolutely impossible, it is an impracticable construction. I have worked it out roughly, and I find that I should require at least as much copper on those reversing pole-pieces as I do on the whole of the rest of the machine—6 cwt. or more of copper—in order not to make more than an addition of 50 per cent. to the $C^2 R$ losses of the machine. What then becomes of the high efficiency, or the saving in weight? for there will be a large increase in the

Mr. Sayers. weight of magnets to provide room for this pole-winding. I think the auxiliary pole suggested by Mr. Evershed is a far more likely thing in the direction of reversing pole-pieces;* but I think even that would be found to be rather difficult to design, partly on account of the space occupied, and partly on account of its short-circuiting the main field. It would be very difficult indeed, if not impossible, to get it in without getting a terrible leakage loss.

Mr. Esson is mistaken about the cost of the steel castings; they are perfectly simple, and the price mentioned by him is 20 per cent. higher than was paid for these castings. I do not agree with Mr. Esson either on the question of alterations to such patterns. They can be, and are, shortened or lengthened at the cost of a few shillings merely, not pounds. Then, as regards the stocking of carcasses, one of the cheapest and best methods of construction is to plane out the slots; and when this is done the unplaned cores and fields can be stocked, just as plain cores are stocked at present. I cannot understand Mr. Esson's statement that he gave up the use of projection armatures on account of the extra cost, for my experience, and that of others, is that they are cheaper and easier to wind than the smooth-core type with driving pins and laminated conductors. Ten years is a long time, and it is probable that if Mr. Esson looked into the matter again, with his present experience, he would come to a different conclusion. If proper methods of construction are adopted, I have no hesitation in saying that the saving in material which may be secured on my principle is accompanied by *a saving in cost of labour*.

I was exceedingly interested in Mr. Parshall's description of his machines. He described machines in which the air gap is not greater than in some machines of my own design; and this method of getting over the sparking difficulty is quite similar to the method adopted years ago in connection with smooth-core machines—that is to say, he stiffens up the field. He tells me he works with an induction of about 20,000 in the iron projections of the armature; and he gets a considerable length of iron at that

* Mr. Swinburne in his well-known paper described a pole-piece having the same characteristic as that now suggested by Mr. Evershed.

induction; the result being that he gets a very stiff field, which is only slightly distorted by the armature current. Under these conditions the amount of movement of brushes will be very much less for a given change of load than in the ordinary long-air-gap machine; but I do not see how the amount of sparking for a given want of adjustment can be less: it seems to me it must be greater, on account of the increased self-induction of the armature sections, as compared with a smooth core. Mr. Sayers.

Reverting to the length of air gap question, in view of Mr. Parshall's work the hitherto accepted proposition that the length of air gap was of primary importance in connection with sparking must be modified so as to mean that the magnetic reluctance of iron and air combined around the path of the cross magnetising force must be great. But whether this is done by lengthening the air gap or by pressing a considerable length of iron up to a very high induction, a large magnetising force is required; so that it is difficult to see that any other advantage is obtained by the use of the projection armature on these lines, besides improved mechanical construction. Mr. Parshall said nothing about the hysteresis and eddy-current losses in the projections of the armatures he refers to. These, it would appear, must be considerably greater than in other machines.

I may perhaps point out that Mr. Parshall's equation for sparkless collection, $L\,c^{10-1} = M$, means the same thing as my empirical formula, $i_s = \dfrac{N}{l \times x \times n}$, or $x = \dfrac{N}{i_s \times l \times n}$; that is, x (which may be written L, as its value is proportional to L) is equal to that fraction of the total field cut effectively by a commutator coil. As Mr. Parshall points out, the greater the coefficient of self-induction of an armature section, the greater the amount of sparking due to a given change of current in the armature. In this connection, I must also point out that, while in machines of the ordinary type, or even in the very ingenious machines of Mr. Parshall, a change of current in the armature always changes the strength of reversing field in the wrong direction; when, as in my machine, the reversal is produced by the flux under the horn that is strengthened by the armature current, a change

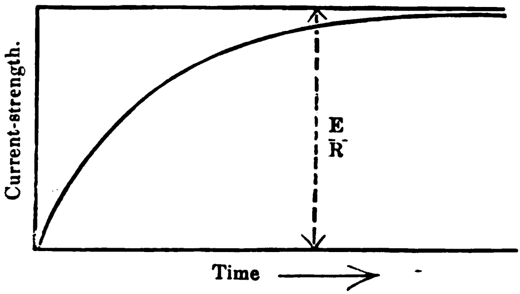
Mr. Sayers. in the armature current is accompanied by a change of field in the proper direction, even if the amount of change is not precisely what is required to maintain sparklessness. So that, with a given coefficient of self-induction, the sparking must be much less for a given change of current when my arrangement is adopted than when the flux under the horn which is weakened is used.

Mr. Evershed thinks that we shall not have reached perfection until we work with a mere mechanical clearance. Well, I have no difficulty in working with 1 mm. as regards sparking. In fact, several machines of my design have been working continuously with this clearance, and without giving trouble of any kind; but for ordinary work it is not desirable to use less than, say, 3-16th inch to one-fourth inch for large machines, on account of the severe side strains which are set up if the armature is a little out of centre. The clearance of the 80-kilowatt machine is made one-fourth inch for mechanical reasons. If we shortened the air gap we should have to contract the opening at top of slots; but the reduced leakage into the opposing commutator coil, owing to the smaller exciting force with reduced gap, is sufficient to make up for the increase of self-induction due to contraction of opening of slot.

Mr. Evershed called attention to the fact that it was an exceedingly nice thing to hit the right point in the current-reversal curve. I may first of all remark that it certainly requires a minimum E.M.F., or perhaps more, certainly a minimum amount of energy, to create a spark of any dimension. I do not know what the E.M.F., or amount of energy required, may be, but it does not follow that because one does not precisely hit the point there will be a spark. A certain amount of energy will be got rid of by the growing resistance of the contact between commutator sector and brush as the latter slides off the former, which Mr. Houston described during the discussion on my former paper. This resistance must be sufficient to dissipate the energy remaining in a coil due to minute and unavoidable variations; consequently, we have a certain amount to come and go on which practice shows is amply sufficient to make absolute sparklessness readily attainable. If we do not get any sparking, the small amount of heat generated

by the narrow contact when the current is small, owing to the commutation being nearly correctly performed, will not damage the commutator. Mr. Sayers

In connection also with what Mr. Evershed said, I may remind you that the curve for a rising current in a coil is in this form:



Curve for rise of current, taken from "The Alternate-Current Transformer," by Dr. J. A. Fleming.

It depends entirely upon the point in that curve at which your commutation is complete. In other words, if the half armature current is nearly equal to $\frac{E_r}{R}$, where E_r is the resultant reversing E.M.F., and R is the resistance of the coils in which the current is changing, then there would be very little tendency to spark with a comparatively large variation due to imperfect dividing of coils or commutator. But, as a matter of fact, I believe, from calculations I have made on the subject, that I am working on the steepest part of the curve; the time during which the commutation is performed being usually only about half, or sometimes less, the time constant of the circuit, or one-third to one-fourth of the ultimate current-strength which would be attained were the resultant reversing E.M.F. applied for sufficient time. But in spite of this I have no doubt that in my machines the current-slope is rounded off very much, as Mr. Evershed suggests it should be, because in practice the brushes are set in the sparkless position, which, of course, is found experimentally in the usual way; and this means that they are set so that the commutator coils are operating to just the required extent, and no more, the E.M.F. generated by one being opposed by the adjacent one

Mr. Sayers. under the brush to just the proper extent. At any rate, the machines run absolutely without any sparking at all; and I may say that, with the alteration I have described in the paper of extending the pole-piece, the position of the brush is not at all sensitive. A 32-kilowatt machine made with this extended pole-piece admits of the brushes being shifted 0·1 of an inch on the commutator without any sign of sparking. A very rough adjustment indeed will fix it within those limits.

In reply to Mr. Snell, I can assure him that the reversing arrangement for motors has never given any trouble. We have it in use both in coal haulage and also in launch motors, and it has never given any trouble whatever. It is so perfectly simple that one fails to see how it could get out of order.

Mr. Addenbrooke argues that it is natural that the brushes should require moving; but I think he overlooks the fact that the increase in armature current produces an increase in the reversing field. It is quite true that in the 80-kilowatt machine the increase was not sufficient to entirely obviate the necessity for moving the brushes; but I have good reason to expect that, with some slight modifications in the design, I shall be able to secure absolute fixity of brush position.

Professor Thompson asked whether any member had run a commutator in oil. I have never done so; but it is within my knowledge that the late Mr. J. E. H. Gordon used a commutator entirely immersed in paraffin oil in connection with the first of the large alternating machines which he made at the Telegraph Construction Company's works. He made an attempt to commutate the currents from those machines into continuous currents. I never saw it working, but Mr. Gordon told me himself that it was a complete failure. I think the oil was thrown all over the place, and it was found quite impossible to do anything with it at all. Then Professor Thompson described a very ingenious arrangement by Messrs. Hutin and Leblanc of surrounding an armature with a copper cage, in order to prevent the angular movement of the armature field, and therefore do away with sparking. I have not done that exactly, but I have done a somewhat similar thing. In order to reduce the self-induction

of an armature section, I place a thick piece of copper close to the armature, just over the position in which current is undergoing reversal, and I have certainly found that effective—not to any very great extent, but it has an appreciable effect in reducing apparent self-induction and allowing a larger current to be reversed. With regard to Messrs. Hutin and Leblanc's device, it seems to me that it would be useless to put it through the magnet poles, as they already form a sufficiently good conductor for any steadying currents which tend to be generated, and the copper would not materially improve it; but, even supposing that the device prevented the motion of the field entirely, I cannot see that sparklessness would follow, as the effect under consideration is only one feature in the causes of sparking. Nor can I understand how such a device can be used in connection with alternating currents without great waste of power and fatal heating. In further reply to Professor Thompson, reversible armatures are required in order to make armatures interchangeable when there are several plants running opposite ways. It is, as Professor Thompson suggests, seldom required to run the same machine in opposite directions, except in train-lighting. In applying the reversible winding to ring armatures I lay the coils on flat, in the same way as for a drum winding. This saves wire, obviously. Mr. Sayers.

There is no difficulty about insulation in the slots, though some speakers have suggested that this would be a difficulty. Some years ago, Mr. Swinburne stated that there was no force exerted on an embedded armature conductor—that the force was experienced by the iron. This, I think, is not absolutely true; but the force experienced is only a small fraction of what it would be if the conductor were laid on a smooth core. It may be as little as 1-200th of what it would be on a similar conductor on a smooth core; but if the induction in the projections is pressed very high—say up to 20,000, as spoken of by Mr. Parshall—then it may be about 1-15th; so that, in any case, it is reduced almost to the vanishing point.

With regard to the question of cost, I do not think the figures asked for would be of much use if I gave them. The value of material is easily calculated on the figures given,

Mr. Sayers. and each maker must make his own estimate of time, according to the facilities at his command and the particular methods of construction adopted. I must say that I am surprised that this cost question is looked at just as it is, because my experience, and the experience of others also, is that these armatures are not more expensive to build than the ordinary type, especially in large machines. In large machines you get rid of the necessity for laminating the conductor and for winding either double or treble, or of going to four or more poles. You can make a large two-pole machine with only 36 sections in the armature, and working with a current of 800 amperes. I do not think anybody has done that before—commutating 800 amperes—that is, 400 amperes in every element of the winding. I think, in a sparkless machine, that has been done by a duplex or triplex winding. This arrangement of mine enables it to be done in a single winding, and therefore we have a few part, cheap commutator, and a few section cheap solid bar winding; and I have not the least doubt that those gentlemen who have called in question this matter of cost, and have stated that the one drawback was that the armatures were more expensive, will find that it is, as I say, quite the opposite, and that the armatures are cheaper to build and not more expensive than the ordinary smooth-core type.

APPENDIX.

As I have made some statements in the paper, and in the foregoing reply to the discussion, to the effect that the weight of the 80-kilowatt machine might be considerably reduced, and that generally the weights of my machines could be very materially reduced, the following may prove of interest.

The empirical formula given in the paper for non-sparking is—

$$i_s = \frac{\mathrm{N}}{l \times x \times n}.$$

Now,

$$\frac{\mathrm{N}}{l \times x \times n} = \frac{\frac{\mathrm{N}}{\sqrt{y}}}{\frac{l}{y} \times x \times (n \times \sqrt{y})};$$

which means that if we reduce the length of core $\frac{1}{y}$th, we may

Mr. Sayers.

reduce N by $\frac{1}{\sqrt{y}}$, multiplying n by $\sqrt{y}$ in order to keep the E.M.F. with a given speed the same. Now, taking $y = 2$, and applying to the 80-kilowatt machine, we get the diameter equals 26 inches; length of core, 10·5 inches; N = 14,600,000; $n = 104$. This alteration, according to my calculations, would result in a saving of about 15 per cent. in cost of material and 18 per cent. of weight. I find, however, that on my present experience I could depend upon securing sparkless reversal with an armature 12·5 inches long and having N = 14,600,000: the diameter then becomes 23 inches, n being 104, as above. The saving in weight as compared with the 21 × 21 machine described in the paper is 26½ per cent., and of cost of material 22·5 per cent.; and I do not see any reason why still further reductions could not be made as experience is gained.

The CHAIRMAN announced that, as the result of the ballot, the following candidates were elected:—

Foreign Member:

Henry Fleetwood Albright.

Associates:

Edwin H. Barton, D.Sc. (London).
Captain F. Baylay, R.E.
Nelson Dadge.
Lieut. A. H. Dumaresq, R.E.
Frank Burridge Foy.
Lieut. M. E. R. Maunsell, R.A.
Herbert Nash.
William Llewelyn Phillips.
Eustace Thomas, B.Sc.

Students:

Frederick John Caley.
Norman Endacott.
Arthur Thomas Gordon-Smith
Reginald Charles Harpur.
Hugh Godfrey Nicholson.
Edward Fairlie Watson.
Norman James Wilson.

The Two Hundred and Seventy-third (extra) Ordinary General Meeting of the Institution was held at the rooms of the Society of Arts, John Street, Adelphi, on Thursday evening, February 21st, 1895—Professor GEORGE FORBES, F.R.S., V.P., in the Chair.

The CHAIRMAN: It has been resolved on the present occasion to dispense with the reading of the minutes and preliminary business. Mr. Crompton, unfortunately, has been unable to attend this evening, as he has been summoned to the Continent. I will now call on Dr. Hopkinson to read the paper by himself and Mr. Wilson on the "Propagation of Magnetisation in Iron."

PROPAGATION OF MAGNETISATION IN IRON.

By Dr. J. HOPKINSON, F.R.S., Past-President, and
E. WILSON, Member.

Dr. Hopkinson and Mr. Wilson.

It is well known to all those who have watched the development of electric lighting, and who have had to handle dynamo machines of ever-increasing size, that the larger dynamo machines of recent times require a much longer time to develop their magnetism than the smaller machines which have now been in general use for many years. Other things equal, self-exciting machines take longer to excite their magnets than is taken to excite the same magnets from an external source of supply. But even when the supply is from outside, and is applied at once with full pressure, an appreciable time is taken before the machine exhibits its full potential. The Siemens machines at King's College have an output of about 10 kilowatts, and when independent excitation is applied take about 12 seconds to become fully magnetised. One of the causes of the slow magnetisation of large machines is the subject of the present paper.

Though the use of the ballistic galvanometer for the purpose of studying dynamo machines is by no means universal, it is, at all events, not unfrequent; and it must be familiar to those who

Dr. Hopkinson and Mr. Wilson.

have used the ballistic galvanometer with large magnets that a difficulty arises in its use when the section of the magnets becomes at all considerable. Even with the galvanometer arranged for a time of oscillation of 12 to 15 seconds, it is found that the first elongation is much greater than the second in the opposite direction, and that often the third is also greater than the second. This is due to the fact that the induced current which we have been in the habit of regarding as instantaneous, in comparison with the time of swing of the galvanometer, is not so, and that it continues of a sensible amount for a time of 15 or 20 seconds or more. If the experiment be tried upon an electro-magnet with a divided core, it is found that the same effect can be observed. But in this case it is not difficult to diminish it to any desired extent, by using a supply of current at a much higher potential than is necessary to create the current through the resistance of the coils of the electro-magnet, and to take up the excess of potential by non-inductive resistances.

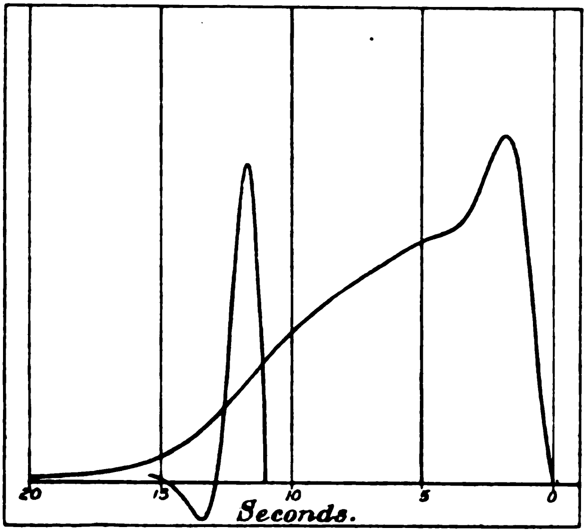


Fig. 1.

Fig. 1 shows the relation between time and current in the secondary of an old Westinghouse transformer when the current in the primary is reversed, first, when the current is given by one cell; second, when the same current is given by 50 cells. The

Dr. Hopkinson and Mr Wilson

reversals occur at different points in the scale of abscissæ, and the sensibility of the galvanometer—a D'Arsonval of Ayrton and Mather's type, made by Messrs. Paul—is different for the two curves.

It is apparent that, whereas the induced current is all over in two seconds when the electro-motive force is 100* volts, it lasts 17 seconds when the electro-motive force is 2 volts, the currents in the two cases being the same. This retardation of the induced current by electro-magnets with divided cores is, of course, due to the self-induction of the coil. It has been made the subject of a good deal of experimental research by Professor Gray (*Trans. Royal Society*, 1894). This is not the subject of the present paper.

If the core of the electro-magnet is not divided, the same effect, it is true, is observed. By increasing the electro-motive force of the magnetising current we are enabled to increase the speed with which changes of magnetisation occur, but only to a limited extent. The reason of this is familiar. Iron, of course, is a fair conductor of electricity, and when the current in the magnetising coils is reversed, opposing currents are excited in the substance of the iron core of the magnet, and these currents for a time prevent the full change in the magnetising force in the interior of the mass.

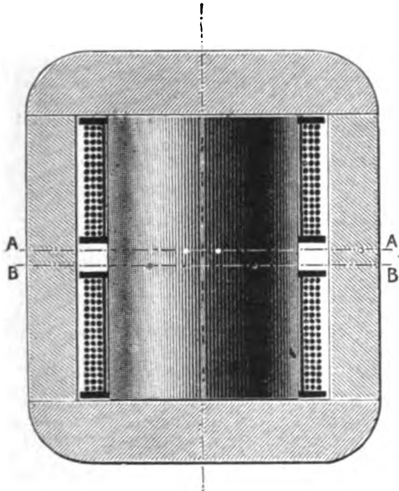
If the induction in iron was proportional to the magnetising force, it would be possible to calculate under any given circumstances the course which the changes of induction would pass through; but this elegant assumption is wholly untrue; and although the mathematical treatment of the subject, if the assumption is made, is not very easy, it is enormously more difficult if the actual facts of the case are assumed. The only way, therefore, is to attack the matter experimentally. This we did a year ago with a magnet of substantial size, and the results are being published in the *Transactions of the Royal Society*.† Since then Messrs. Mather & Platt have constructed for us a much larger magnet, capable of showing the results in a more striking

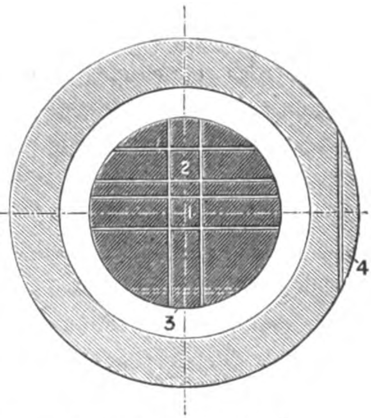
* The dip below the line of abscissæ is due to the swing of the galvanometer.

† *Phil. Trans.*, vol. clxxxvi. (1895), A, p. 93.

form. The present magnet consists of four pieces, shown in Figs. 2 and 3. It consists of a central circular cylinder 12 inches

Dr. Hopkinson and Mr. Wilson.





Scale 1 in. to 1 foot.

FIG. 2. FIG. 3.

in diameter; next, a ring of the same length as the cylinder, surrounding it; and, lastly, two wrought-iron slabs completing the magnetic circuit. The magnet is excited by copper coils wound in the annular space between the solid cylinder and the ring. In the central cylinder are wound three exploring coils. Firstly, we have one (No. 3) which encloses within it only portions of the iron near to the surface. This is obtained by simply drilling a hole in the iron and winding the coils through it. Second, we have a coil (No. 2) square in form, the side being about 2 inches, and located in the mass at a depth half-way between the surface and the centre of the cylinder; and, lastly, we have a coil (No. 1) of similar dimensions round the centre of the cylinder. The spaces in which the coils are wound are holes about 5-16ths of an inch in diameter, drilled through the substance of the iron. Through these holes a strand of 19 insulated wires is drawn, and the ends of these wires are finally connected up so that the 19 convolutions are in series. We have thus buried in the solid mass of the iron three exploring coils capable of indicating the change of induction which occurs

Dr. Hopkinson and Mr. Wilson.

through them. In addition to the three coils already mentioned, we have a fourth coil (No. 4) in the outer cylinder, for the purpose of showing the way in which the magnetisation changes in such a position.

In our first apparatus we adopted what would appear to be the most obvious expedient of cutting out grooves in the faces betweeen two iron surfaces and winding the coils in these grooves. But it was soon found that this could not give accurate results, and the reason of the difficulty, when it is once experienced, is not far to seek (see paper in *Phil. Trans.*). Suppose that there is between the two faces a minute space not occupied by iron; imagine a closed curve drawn in each of the two masses intended to be in contact, and running throughout its length near to their surface. It is clear that the line integral of magnetising force due to currents in the iron round this curve can be made as small as we please, and that if the iron is far from saturation the principal part of the magnetising force will be taken up in the non-magnetic space between the two surfaces. There will, therefore, be a very considerable tendency to equalise the induction over the surfaces in contact.

In order to avoid the effects of self-induction, and to limit the observations to observations of the effect of local currents in the iron, a considerably greater battery power is used than is required to excite the currents in the magnets—about 100 volts—the excess being taken up by practically non-inductive resistances, consisting of lamps. The current then from the battery is taken through the non-inductive resistance to a reversing switch, which, in reversing the current through the magnet, short-circuits it and does not break it, so avoiding destructive sparks at the switch. The amount of this current can be measured by the difference of potential across the non-inductive resistance. The exploring coil in which it is desired to measure the electro-motive force is connected, through appropriate resistances for varying sensibility, to the D'Arsonval galvanometer, which throws its image upon a sensitive plate which is steadily moved at an appropriate speed in a direction perpendicular to the deflection of the image.* We

* The photographic box and clockwork were made by Mr. Groves.

thus obtain a curve connecting the electro-motive force with the time. The three curves for the three coils are superimposed on the same plate for better comparison. The ordinates then measure at any moment the electro-motive force round the square, or area, enclosed by the connected coil. They therefore measure the rate of change of induction in this area. The currents induced in the iron take an astonishingly long time to entirely die out. Figs. 4 to 8 (Plate A) show the results of experiments with five different magnetising forces on the inner cylinder, whilst Fig. 9 shows an experiment on the No. 4 coil of the annular part of the magnet. In all these figures, times increase from right to left, and are given by the numerals in seconds. Dr. Hopkinson and Mr. Wilson.

In order to compare the sensibilities of the galvanometer as connected to the different coils, the areas of the curves may be taken. These areas should of course be made equal by multiplying the scales of ordinants of one or the other curves, for the total amplitude of change of induction is sensibly the same at all parts of the magnet. Fig. 4 gives the results for coils Nos. 2 and 1 with a force of 1·2, the more quickly rising curve being No. 2. In this case the sensibilities for the two curves are sensibly the same. It will be noticed that there is but a single maximum of rate of change of induction, and that the maximum for the intermediate, or No. 2, coil is much greater than the maximum for the No. 1, or central, coil. It is also important to notice that the maxima of both coils occur comparatively early; in No. 2 coil at about 15 seconds after reversal. Fig. 5 gives the results for all three coils with a force of 2·4. The scale for No. 2 and No. 1 coils is the same; that for No. 3 coil is much less sensitive. It will be noticed that the changes of induction in No. 3 coil take place almost instantaneously, and everything is practically over in three seconds. No. 1 coil attains its maximum very much later than when the force was 1·2. The fact is, we have in this case two maxima—the first maximum, which really corresponds with the maximum with the smaller force; and a second, and greater, maximum, which develops later. Note also that in this case the maximum for No. 1 coil is smaller than the maximum for No. 2 coil. Fig. 6 gives similar results, the magnetising force being 6. But in

Dr. Hopkinson and Mr. Wilson.

this case the scales are different for the three curves, No. 1 coil having a less sensitive scale than No. 2. In fact, the maximum for No. 1 coil is greater than the maximum for No. 2 coil—unlike the result with a force of 2·4. Note further that the first, or subsidiary, maximum is more marked than with the smaller force, and that the second maximum is occurring earlier. Fig. 7 gives the same results with a force of 11. Again the maxima are occurring earlier; in fact, Fig. 7 differs from Fig. 6 in the same way as Fig. 6 differs from Fig. 5. Fig. 8* in the same way gives results for a force of 24; and again the results differ from Fig. 7, just as Fig. 7 differs from Fig. 6. Note also in Fig. 8 how suddenly the curve drops from its final maximum. In coil No. 1, the rate of change of induction seems to alter almost instantaneously from its greatest value to *nil*. Fig. 9 gives the curve of electro-motive force for No. 4 coil in the annular part of the magnet for a force of 24. It serves to show that the general character of the phenomenon is the same. The change of induction begins at the inner surface of the annulus, and spreads outwards.

To put the matter shortly, then, what we find is that with the medium and large forces two maxima are observed in No. 2 and No. 1 coils—a smaller one, which occurs early; and a greater one, which occurs late. But with a very small force the earlier maximum is greatly increased, and the second maximum no longer appears as a maximum at all. Further, that with a small force the maximum is obtained comparatively early, and with a somewhat larger force the second maximum occurs very late, and that as the force is further increased the maximum occurs continuously earlier; that in all cases the disturbance in No. 1 coil is almost instantaneous and occurs at once, No. 2 coil succeeds it, and No. 3 coil occurs much later. With a force of 6 and upwards the maximum in No. 1 coil is materially greater than the maximum in No. 2 coil, but that the change occurs much more suddenly. That these results are not other than we should expect, we have shown in the Royal Society paper in this way. In that case, instead of coils of equal area at different depths, we

* There is a small displacement of the point at which the curve of No. 1 coil begins.

had circular coils, the centre of the circles being at the centre of the mass. The currents in these coils therefore give the measure of the electro-motive force, and from the resistance of the iron we calculated the currents at different depths; and hence, by an interpolation, we were able to estimate at any time the electro-motive force at any depth in the iron. By integrating the curve of electro-motive force with respect to the time we can also estimate the induction at any depth, and we could then express by a curve the relation between induction and magnetising force. Of course the curve so obtained cannot be expected to be by any means accurate, as from a knowledge of the electro-motive force at the three depths it is difficult to estimate it at all depths. Still, we do find a very fair agreement between the curves obtained from these experiments and the well-known cyclic curves for iron.

Dr. Hopkinson and Mr. Wilson.

The general character of the results can be explained in another way. Fig. 10 (Plate A) shows cyclic curves of induction for three magnetising forces—a low force, an intermediate force, and a high force. In the low force the scale of abscissæ is different from the other two, and it will be observed that the ratio of change of induction to change of magnetising force is small, and comparatively uniform. We should therefore expect that, just as in a telegraph cable of low resistance the transmission is rapid, here the magnetic disturbances would pass in comparatively rapidly; and so we find it to be, as shown in curve 4. With the intermediate force we find that the mean permeability—that is, the ratio of the total change of induction to the total change of magnetising force—is great, and therefore that the main part of the disturbances passes into the mass slowly. We further find that at the beginning of the change the permeability is small. This portion of the change then would pass in rapidly, and give us, in fact, our first, or subsidiary, maximum. The permeability then becomes very great, as is indicated by the almost vertical portion of the curve, after the major portion of the change in magnetising force has occurred. These changes will pass in slowly, and they give rise to the second maximum. Lastly, for great forces the changes are characterised by a small permeability at first,

Dr. Hopkinson and Mr. Wilson.

then a large permeability, and, lastly, a small permeability again; the average permeability being again small. The result of this is a first, and clearly defined, maximum, followed by a greater maximum, which disappears almost suddenly in No. 1 coil, owing to the way in which the final part of the wave of induction presses upon and almost overtakes the more slowly advancing portion in front of it.

The results of these experiments have a much wider application than at first sight appears. From them we can infer what will happen in cylinders of other sizes than those upon which the experiments have been made. Consider two cylinders, one double the size of the other, magnetised to the same intensity of induction with the same magnetising force. Consider in these cylinders two annuli, one double the diameter of the other, and double the thickness in a radial direction, but of the same depth in the direction of the axis. The resistance of these annuli will be the same, because their area of section and their lengths are both in the ratio of 1 to 2. The induction through these annuli are as the areas—that is, as 1 is to 4; hence, if the inductions vary at rates having to each other the ratio of 4 to 1, the currents in the iron between corresponding radii will be the same at times having to each other the ratio of 1 to 4, and the magnetising force will also be the same. Hence we see that corresponding events will happen in cylinders of different sizes, but at times proportional to the square of their linear dimensions. We are thus able to infer what will happen with magnets of other sizes. For example, take the case of a magnet 1 inch in diameter, and with a magnetising force of 24. The events will be the same as in our last curve, but will occur in a time 1-144th as long as those which we have observed. In our experiments it is all over in 40 seconds; and hence in an inch bar we should expect all to be over in less than one-third of a second. The same, of course, applies to smaller sections—for example, to the wires which are sometimes used in transformers. Take the case of a wire 1-25th of an inch in diameter. Everything will happen in exactly the same way as in our experiments, but 90,000 times as fast; and hence we should expect that all changes in the iron would be complete in

about a 2,000th part of a second if the force were 24, or in 1-200th part of a second if the force were $2\frac{1}{4}$. This brings us to the second part of our subject. Dr. Hopkinson and Mr. Wilson.

There has been abundance of discussion about the effects of local currents in the iron of transformers, and about the amount of subdivision which it is worth while to give to them. The results so far explained throw no complete light upon the question; but it is easy enough to modify the experiments so that they shall. We have only to make use of alternating currents of a suitable frequency in the magnetising circuit. Currents, of course, of such low frequency cannot be conveniently obtained by induction. The more convenient plan is to obtain them by suitably varying resistances in the circuit. We have used a current reverser substantially similar to one which has already been described by Professor Ewing. The current is taken through a bath of solution of sulphate of copper between two plates; and between these two plates revolve two segments of a cylinder, and these two segments are connected to our electro-magnet. As the segments revolve they approximate more or less nearly to the potential of the two fixed terminals alternately, and vary continuously between these values, giving an approximation to a sine curve of currents. The currents are measured by measurement of the potential difference between the two ends of a small non-inductive resistance. The arrangement is shown in Fig. 11. We give the results of three experiments made upon the 4-inch magnet of the Royal Society paper. In comparing these with the results already given, remember that in this case No. 3 coil embraces the whole core, instead of only a sample near the surface; No. 2 coil, all within a $2\frac{1}{2}$-inch radius, instead of only a sample at about half-way from centre to circumference. The magnetising force due to current in the copper coils is in the first two cases 22. The periodic time in Fig. 12 (Plate A) is 80 seconds; in Fig. 13 it is 20 seconds. Comparing the innermost and outermost coils, we see that the rate of change in induction in the innermost coil is of about equal maximum value in the innermost coil, but is twice as great with a period of 20 seconds as for a period of 80 seconds in the outermost coil. The time lag for the

Dr. Hopkinson and Mr. Wilson.

innermost coil is about one-sixth of a period for 80 seconds, but is 5-12ths of a period for 20 seconds. Fig. 14 gives the results for

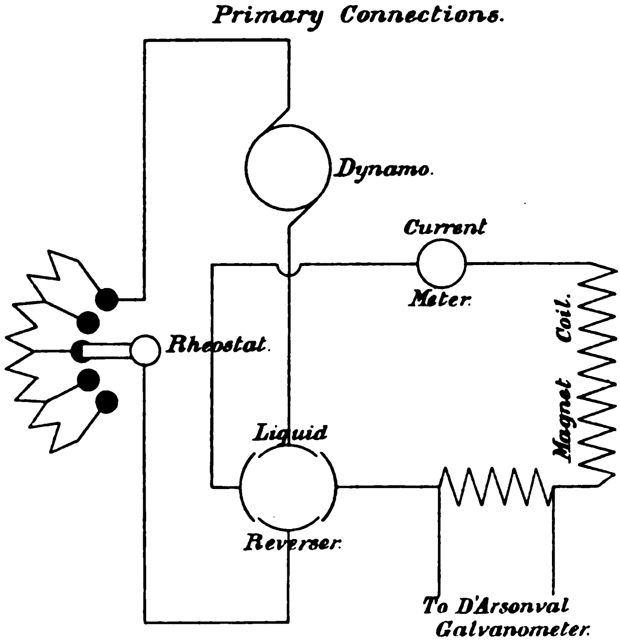


FIG. 11.

a force of 5 and a period of four seconds. No. 2 coil lags much more than it did before, and the amplitude of the innermost coil is now so small as to be almost inappreciable.

In exactly the same way as has already been explained, the changes in smaller cylinders will go on in the same way as in our large cylinders, but at times more rapid in the inverse ratio of the squares of the linear dimensions. Take for example a millimetre wire. The same things will happen, but ten thousand times as fast Hence the interior of the millimetre wire would be quite useless if there were so many as a thousand periods per second. An examination of the results shows that the sizes now generally used in transformers are about right. They could not be greatly increased without loss, and to diminish them would be needless.

Having dealt with the practical aspect of the question, we may perhaps enter on a wild speculation. Suppose a magnet

such as we have here constructed, but of the dimensions of the earth, and that some almighty electrician reversed its currents in the copper coils, the magnetising force being 2 or 3. It would take some thousands of millions of years before the rate of change of induction at the centre of its core would attain a maximum. We throw it out as a suggestion to those who enjoy such speculations, that the earth's magnetism is due to currents in its material, sustained by its changing induction, but slowly dying away. However, we should not like it to be supposed that we believe the suggéstion to contain a particle of truth. Dr. Hopkinson and Mr. Wilson.

The CHAIRMAN: I am sure, gentlemen, when the time comes to ask for a vote of thanks to Dr. Hopkinson and Mr. Wilson, you will all join very heartily in that vote. In the meantime, we shall be glad to hear the remarks of members on the extremely interesting paper we have heard.

Dr. J. A. FLEMING: We never hear Dr. Hopkinson on any occasion without hearing him to our own profit and pleasure; and I am sure that I only express the feeling in the mind of those present when I say that we have seen with much interest the experiments he has shown us, and have listened to the lucid descriptions of them given by him. When we see experiments of this kind shown to us, illustrating so clearly what we have previously understood perhaps in an uncertain manner, it throws a flood of light upon our whole past experience. I remember trying an experiment at the Edison & Swan works at Ponder's End with the large field magnets of an old dynamo. On one occasion we joined up those iron field cores in series and passed a current from a secondary battery through these coils to magnetise these field magnets, and we had a shunted galvanometer placed in series. I recollect distinctly our astonishment at finding that the current required some six or seven seconds to become steady. Those field magnets, if I recollect rightly, were something like 6 or 7 inches in diameter. Although I do not remember the magnetising current, that figure agrees fairly well with the number that Dr. Hopkinson has given us. I think he told us in the magnet 12 inches in diameter the whole of the magnetic events in the interior were over in about 40 seconds. If we Dr. Fleming.

Dr. Fleming. take the square root of that, it is 6 or $6\frac{1}{2}$, so that in field magnets 6 inches in diameter the magnetic events would be all over in about six or seven seconds; and that was practically the case. Although these experiments open to us many suggestive lines of thought, I must not venture to occupy too much time over them. One thing they naturally do is to connect together in our own minds forcibly the mode in which a current increases in a wire, and the mode in which the magnetic induction increases in a core. It has been shown that these two things are from a mathematical point of view identical in behaviour; and here we see shown most admirably upon the screen the manner in which this induction slowly creeps up from point to point as it travels into the core, and that if the induction is being rapidly reversed on the surface it is being reversed in the different layers of the core underneath, but not at the same time; and, as a matter of fact, in a core of sufficient thickness it never gets into the centre at all. That is a very familiar fact from another point of view in connection with the alternating current. We know perfectly well if we use very large solid conductors to convey alternating current the central portion of the conductor is not used: it is useless to employ solid conductors of above a certain diameter. That is perfectly well understood in connection with alternating-current work. Dr. Hopkinson has explained the dependence of this rate of propagation inwards of the induction upon the permeability. I am sure, therefore, we shall not only digest all that Dr. Hopkinson has told us, and so well illustrated, but it will open up to our minds a large region of fresh thought which we shall enter on with great pleasure.

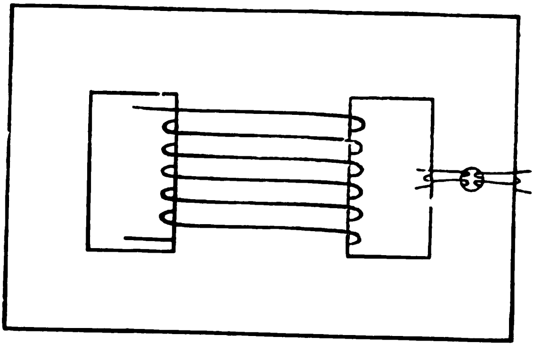
Captain Creak. Captain CREAK, R.N.: I am afraid I have not very much to say with regard to this valuable and remarkably clear paper. When I came in it was suggested to me I should offer some criticisms upon it. I confess I have remained here to learn a great deal. There is, however, one point on which I may say a few words, and that is with regard to some experiences of my own as to the time which magnetism takes to propagate through the dynamos on board H.M. ships.

I daresay many of the members may know perfectly well

that if a dynamo of the usual types be mounted on board ship within a certain distance of the compass, that compass is disturbed. We have had a number of ships in which we have had a pair of dynamos, symmetrically placed with regard to the compasses, one on each side of the ship; and the action of this arrangement has been that, supposing we ran the port dynamo at night, and the engineer found it was not working well, and then used the starboard one instead, we should have a change of some 4 degrees in the compass course, which, at the rate of 19 knots, would place the ship about 10 miles out of position in an eight hours' run. However, we have so far got over this that, supposing we are running the port dynamo, we are enabled, by an arrangement of switches adopted by the Admiralty electrician, to use the starboard dynamo as a corrector. The correction is absolute; but what I want to point out, in confirmation of what Dr. Hopkinson has said, is, that when I go to see that the switch is doing its work properly, I find I have to wait about four minutes before satisfactory action takes place. Thus, at first the compass is in a state of agitation for three or four minutes, giving uncertain results to the observations of compass errors; and then the equilibrium between the dynamo and corrector settles down, and the correction is exact. I would remark that, though from the foregoing experiments I dimly saw some of the points which Dr. Hopkinson has so ably explained to-night, a flood of light has now been thrown upon them, and I see things very much more clearly since witnessing the experiments which have given us all so much pleasure. Captain Creak.

Mr. MORDEY: It is impossible to discuss a paper of this sort adequately on a first hearing. It throws, as Captain Creak has said, a great deal of light on many things with which a good many of us have been familiar, but have not been fully able to explain. I was interested particularly to hear a confirmation of a practice which we all drifted into in the matter of transformer design. Dr. Hopkinson finds that the degree of lamination that has been adopted has been confirmed by his researches. As bearing on the penetration of magnetism into laminated structures, I may mention that some five years ago I made some experiments with Mr. Mordey.

Mr. Mordey. the object of finding whether magnetic penetration in laminated iron took place evenly through the whole area of the core or sheath of a transformer. I used a method something like the authors'. A hole was drilled through the sheath at right angles to the laminations, and wires, threaded through the hole, were arranged to enclose different parts of the section of the sheath, as in the figure below. The transformer was then magnetised by an alternate current in the usual way and the search coils connected to a voltmeter. It was found that the outer portion of the sheath was magnetised to the same density as the inner, although the magnetic circuit of the former was larger. I explained this at the time by supposing the tendency to unequal distribution in the different portions was counteracted by the changes in permeability.



Another engineering matter on which light is shed by Dr. Hopkinson's results is that of the excitation of electro-magnets by a redressed, or pulsating, current, as distinguished from an alternate current. On a few occasions I have designed alternators with large solid cores, the magnets of which were excited by alternate currents, redressed by an ordinary commutator. I must confess that I expected to find the arrangement wasteful, and I started with the idea that we should probably have to laminate the magnet sooner or later if we wanted to make it efficient. But I found that was not the case. I found that it required the same current—measured by a Siemens dynamometer—to excite it. That I was prepared for. But I also found that the power, with a redressed current, was very little higher than with a direct current. There was an effect

of self-induction; but it was an effect which scarcely showed itself in the watts actually absorbed by the magnet. The power-factor was about 0·8, rising slightly with the amount of current. Apparently the interior of the magnet settles down into a staple state under a very slight surface fluctuation, which suffices to prevent the penetration of pulsations. That is the kind of explanation which occurred to me at the time, and I should like to know whether Dr. Hopkinson agrees with it. It is a part of the subject that is very likely to become of engineering importance. Mr. Morley.

Mr. EVERSHED: There is one feature about this interesting paper to which I should like to direct your attention, namely, that the problem which Dr. Hopkinson has solved for us has hitherto only been attacked mathematically. The penetration of induction to the centre of an iron core was discussed some years ago by Professor J. J. Thomson in the *Electrician*, and, as in all such cases, it was necessary, in order to make the problem a practical one for mathematics, to assume that the permeability of the iron was constant. Naturally, the solution was hardly satisfactory to those who have to deal with iron as it is—a material with many perplexing qualities, and not a constant one among them. Dr. Hopkinson, however, has always gone on totally different lines in his work, and I think we should all of us feel grateful to him for showing us such an excellent example of sound blending of theory and experiment. Many years ago, while most electrical people were attempting to predetermine the characteristic of the dynamo by some mathematical function more or less complex, Dr. Hopkinson threw over all empirical formulæ, and appealed directly to Nature. Taking the curve of the magnetisation of iron, he was able to show us how to design a dynamo from a knowledge of the magnetic nature of the iron used; and since then we have all been designing dynamos in that way. Now, Dr. Hopkinson deals with this far more difficult question precisely in the same manner, and with same success. When you have a material, like iron, which has a most complex law with regard to magnetisation—a law which, so far as one can see, cannot possibly be treated mathematically— Mr. Evershed.

Mr. Evershed. the only thing to do is to obtain the data you want by purely experimental means, as Dr. Hopkinson has shown us this evening. Having obtained in the course of his research a sufficient number of results—as he no doubt has done, although he has only been able to show a few of them to-night—he is able to make perfectly legitimate mathematical deductions from those results; and to show, for example, how thin a transformer plate ought to be in order that the induction may penetrate to the centre of each lamina, how thin the core plates of an armature ought to be, and so on. I think that the *method* of the paper should be impressed forcibly on our minds this evening as a suggestion and encouragement to all younger experimenters to follow on the lines adopted by Dr. Hopkinson when they have to deal with similar problems.

Mr. Addenbrooke. Mr. ADDENBROOKE: I think Dr. Hopkinson's experiments bear in an important manner on the design of alternators. We know that the parallel running of alternators is largely dependent on the induction between the armatures of the field magnets, and this induction is dependent on the permeability of the magnetic circuit. I fancy it has generally been assumed hitherto that the permeability of the magnetic circuit is the ordinary permeability which you would obtain in calculating the magnetic strength of the field; but it is evident that, wherever you have large masses of iron or steel in the field magnets of an alternator, the induction cannot rise to the amount that we would anticipate in the ordinary way, and that therefore the reaction on the armature of the field magnet cannot be quite so great as otherwise might be anticipated.

Professor Forbes. The CHAIRMAN (Prof. George Forbes): I should like to make a few remarks on this most valuable and interesting paper. The subject is one of very great difficulty to attack theoretically. The difficulty of the problem lies in the fact that iron acts in a two-fold manner—first, as a conductor, if I may say so, of magnetism; and, secondly, as a conductor of electricity; and that the induced currents are generated in the iron, in an annular form, concentric with the coils, and that the *locus* of the induced current is actually approaching the centre. The most striking

Professor Forbes.

part of the whole of the experiment, to my mind, was the way in which the No. 1 coil did not move under the influence of changes in those induced currents surrounding it until the effect of these induced currents on No. 2 had ceased—*i.e.*, that the outer boundary, so to say, of the ring of induced current which was contracting was a sharply defined boundary, and had passed through No. 2 before it began to touch No. 1 very sharply indeed. I think Dr. Hopkinson will agree with me that that is a correct interpretation of the phenomena; if not, I would like to be corrected. If so, it is a most remarkable thing to see the sharpness of division. It is useless to dilate upon the importance of the various conditions that have been submitted for our inspection this evening, the variations depending upon the different initial electro-motive forces, and inductions and variations depending upon the linear dimensions. It is extremely important to have learnt from Dr. Hopkinson the fact that if we make an experiment upon a magnet of any sort, and get certain definite results as to the time taken for certain phenomena to be accomplished, we may from that experiment foretell the time that will be taken to go through the same phenomena with a magnet ever so much bigger, or ever so much smaller, it being simply in proportion to the square of the linear dimensions. It is quite impossible, as Mr. Mordey says, to enter very fully on the points in this paper: they are more food for thought for a considerable time afterwards; although Dr. Hopkinson was kind enough in some of his experiments to give us plenty of time to be thinking over some of the important things that were going on. The remarks of Captain Creak about the effect of the dynamos upon a ship's compasses makes me inclined to wonder why it is that Her Majesty's ships should not be supplied with dynamos of what is commonly called the "iron-clad" type—that is to say, dynamos with the magnetising coil round the armature, where the magnetism is wanted, instead of round the field, where it is not wanted; in which case the iron completely surrounding the armature contains all the lines of force, and there is absolutely no field outside, and no compass needle would be affected by the lines of force created. Such dynamos, as Captain Creak is aware, have

Professor Forbes.

been largely made, and I take this opportunity of paying a tribute to the memory of him who developed this design—the late Rudolph Eickemeyer, who, although not an electrician in the early days of his life, was one of the most painstaking and far-seeing of American electricians. I will now call on Dr. Hopkinson to make any reply to the remarks that have been made.

Dr. Hopkinson.

Dr HOPKINSON: First of all, with regard to the points which Mr. Mordey raised. He spoke of the case of a rectified current in the magnet of an alternating-current machine. No doubt he is absolutely right; the effect is simply due to this—that the variations in current in the magnetising coils are compensated by the alternating currents induced in the outside of the iron. He mentioned also the experiment of Professor Hughes. Unfortunately, I can say little or nothing about that, because I have never myself repeated it. I should like particularly to thank Mr. Evershed for his all too kind remarks with regard to myself and my methods of work. However, I do think he is right that the proper way to use mathematics is not to state the facts so that mathematics may deal with them, but to take the mathematics and go upon the facts as they actually are. Of course, when this paper comes to be printed, it will have to be dealt with in a very different way from that in which I have dealt with it here. We cannot refer to experiments which cannot be conveniently put upon paper, and what I propose to do is to photograph the curves; and I have an apparatus all arranged for doing it made by Mr. Groves, whom all of you know probably. I should also wish to mention that the galvanometers we have been using are all of them of the Ayrton and Mather type, and have been manufactured by Mr. Paul. There is one thing always strikes me about matters of this kind, and that is this: that when there is any little bit of credit—I do not know that there is very much in this—connected with work of this kind, it seems all to go to the man who, being senior, is regarded as the principal. Now I do not want that to be the case here, because, as a matter of fact, not only has the labour of preparing experiments fallen upon those whom you will forgive me for calling my lieutenants

in the matter, but a good deal of the initiation of it has also. Dr. Hopkinson. The ideas are not all my own by any means, and Mr. Wilson has contributed not only the labour, but has added a great deal of original thought to it as well. In fact, we have been so much in conference together about it, that I do not think either Mr. Wilson or myself could sort out from this matter and say which ideas are his and which are mine. We have also had help from the student demonstrators in King's College, and the zeal with which they have worked has been beyond praise. I should like to mention, in connection with the work of the Royal Society paper, the names of Mr. Atchison, Mr. Brazil, and Mr. Greenham: they were the student demonstrators a year ago. And with regard to preparing the experiments with the present magnets, I would mention the names of Mr. Cobb, Mr. Evans, and Mr. Macovikski.

The CHAIRMAN: It is my pleasing duty now to propose a hearty vote of thanks to Dr. Hopkinson and to Mr. Wilson for the paper which has just been read, giving us the results of their valuable researches on this most interesting and important subject. After the encomiums that have been passed upon the lecturer by the various speakers, it is needless for me to enlarge upon that subject. I simply propose a vote of thanks to Dr. Hopkinson and Mr. Wilson.

Mr. FRANK BAILEY seconded the motion, which was carried by acclamation.

The CHAIRMAN: I have now the pleasure to propose—"That the "best thanks of the Institution of Electrical Engineers are due to "the Council of the Society of Arts for having so kindly permitted "this meeting to be held in their lecture hall, and for affording "all the necessary facilities for the experiments shown this "evening." Owing to the state of the premises of the Institution of Civil Engineers—which, as you are all aware, are in course of reconstruction—there were difficulties in the way of our having the use of the electric current for illustrative purposes; and had it not been for the kindness of the Council of the Society of Arts in allowing us to have the use of this room, we should probably have been deprived of the pleasure and advantage of hearing the admirable lecture which has just been delivered.

Dr. Du Riche Preller: I have great pleasure in seconding this motion, and, in doing so, I hope I may be allowed to express a wish, and also a fervent hope, that the time is not far distant when we may possess a lecture hall of our own.

The resolution was carried unanimously.

The Chairman: I have another duty—also a very pleasant one—to perform, namely, to propose—"That the cordial thanks of the "Institution of Electrical Engineers are due to Sir Henry Trueman "Wood, and to the members of his staff, for the very kind "assistance they have rendered to the lecturer in making the "necessary arrangements for the experiments." I am sure Dr. Hopkinson must have found the assistance of this staff of very great value, as everyone does who has occasion to give a lecture at the Society of Arts. I call on Dr. Hopkinson to second the motion.

Dr. Hopkinson: I have the greatest pleasure in seconding this motion. I have had nothing but help here; whatever we have wanted we have found, and we have found it in a degree which we could scarcely have expected. I should like in particular to mention Mr. Davenport. He has worked the arc lamps for us this evening—he has two of his own—and in that part of the experimental work I daresay you all noticed that we had not a hitch of any sort or kind whatever; so that to Mr. Davenport we are exceedingly indebted; for I am sure Mr. Wilson quite concurs with me that he has helped us very greatly indeed.

The motion was enthusiastically carried.

The meeting then adjourned.

ABSTRACTS.

ANON.—THE MAGNETIC SEPARATION OF IRON FROM ZINC ORE—FERRARES PROCESS.

(*Eclairage Electrique*, *Vol.* 1, 1894, *No.* 14, *p.* 648.)

M. E. Ferrares employed this process in a mine at Monteponi, in Sardinia, to utilise a waste product which was hitherto of no commercial value, although containing 26 per cent. of zinc ore and about 10 per cent. of iron. It was found that a magnetic oxide could be obtained by converting $Fe^2 O^3$ into $Fe^3 O^4$, by heating it.

The ore is introduced through a funnel into the furnace, and is then carried by means of buckets to a sieve, which separates it into five different sizes. The four smaller sizes fall directly on to the electro-magnets; the larger sizes, more than 5 millimetres in diameter, being crushed to the right size.

These electro-magnets separate the iron from the zinc, the two metals being thrown into separate compartments.

The furnace consists of a rotating iron cylinder lined with fire-brick. The dimensions of the cylinder are 10 metres long, 1·5 metres diameter; it is inclined, and makes 16 revolutions per minute. Each of the three cylinders can roast 12 tons of ore per day of 24 hours with an expenditure of 2 tons of coal. The bottom of the cylinder is maintained at a temperature of 2,000° C. The electro-magnet consists of 24 radial magnets of alternate polarity.

The first part of this plant was started in 1890, and 5,000 tons of waste products were treated, yielding 1,530 tons of concentrated products and 41·7 per cent. of zinc.

M. P. BOUCHEROT—THE ELECTRIC LIGHTING AND TRANSMISSION OF POWER BY POLYPHASE CURRENTS IN MESSRS. WEYHER & RICHEMOND'S WORKS.

(*Bulletin de la Société Internationale des Electriciens*, *Vol.* 11, *No.* 113, *p.* 482.)

The author first points out the two distinct advantages of employing electricity as a motive power for driving factories. In the first place, large steam units are more efficient than small ones, and, for equal power, cost less. In the second place, the losses incurred by heavy mechanical transmission are done away with. In Messrs. Weyher & Richemond's works this second advantage could, however, only be partially realised, as electro-motors were only employed for driving newly installed machine tools.

After due consideration, it was decided that the system of distribution should be with two phases at a pressure of 110 volts. In deciding whether alternating or continuous currents should be used, the following points had to be considered:—

1. The available capital.
2. Efficiency of the system.
3. Practical advantages and disadvantages.
4. Cost of maintenance.

Two-phase currents were chosen in preference to simple alternating currents, for the following reasons:—

1. Polyphase motors and generators have a higher specific output and efficiency than single-phase machines.
2. Polyphase motors are more easily started than single-phase motors, the initial torque being about 10 times greater. Moreover, on constant potential circuits the torque with polyphase motors varies inversely as the speed, and are not subject to sudden stoppage when overloaded, as is the case with single-phase motors.

The speed of the former may be varied by the introduction of resistances in the armature circuit, but not so with the latter.

Two-phase currents were adopted in preference to three-phase currents, for the following reasons:—

1. Two-phase currents are better adapted for the combination of motive power and lighting. The variations of voltage with load are much smaller with the two-phase than with the three-phase system. With the former, if the two sides be fairly well balanced, and one side be fully loaded and the other side half loaded, the variation in voltage will not be more than 2 or 3 per cent.
2. The cost of construction is cheaper with the two-phase system for both motors and generators.

The shops consist of several buildings, covering an area of about 25,000 kilometres, and were formerly driven by three steam engines of 80, 120, and 50 H.P.

The new installation consists of a group of five boilers, of which three are used for running the engine, and the other two for testing engines and dynamos. The main engine is of the horizontal single-cylinder type. Its maximum output is 400 H.P. at 60 revolutions per minute, and its most economical load is 200 H.P. This engine drives a countershaft, driving—

(1) One continuous-current dynamo with an output of 110 volts, 100 amperes, used for exciting the alternators, and of which 50 amperes are available for testing purposes.

(2) A small three-phase machine, used for working an overhead traveller and for testing three-phase motors.

(3) Three alternators of 130 H.P. each, one of which is fitted with a magnetic coupling.

The above countershaft is extended into the dynamo-testing shop, in order that another engine may be used in the event of the large engine breaking down.

The three 80-kilowatt alternators are each capable of delivering 400 amperes per phase, and work at 40 periods per second. They have 12 external stationary poles.

The following is a comparison between the above machines and continuous-current machines manufactured by the same firm:—

1. *Price.*—The price, including the exciter, is 10 to 20 per cent. lower than that of continuous-current machines, on account of their great simplicity and lightness: the weight of the above machines being about 4,500 kilogrammes.

2. Their *efficiency* is about the same as that of a continuous-current machine. The various losses in the above machines were ascertained by driving from a continuous-current motor. The results of the test are given in the following table:—

	At Full Load.	At Half Load.	At Tenth Load.
Armature losses	2,280	570	20
Field-magnet losses	1,520	1,460	1,400
Friction	640	640	640
Windage	1,000	1,000	1,000
Hysteresis...	1,160	1,130	1,100
Internal and Foucault currents	1,760	1,700	1,600
Useful power	88,000	44,000	8,800
Total power	96,360	50,500	14,560
Efficiency	0·913	0·872	0·605

By reducing the windage of the machine the efficiencies were increased to

	0·93	0·89	0·65

If the efficiency of the exciter be considered, the figure 0·913 becomes 0·912.

3. *Practical Advantages and Disadvantages.*—The usual objections raised against the use of alternators is their large drop in voltage, and also the difficulties of running in parallel. During the four months that these machines have been working no difficulties whatever have been experienced. The synchronising current between two machines does not exceed 50 amperes. Continuous-current machines with a larger synchronising current than this would no doubt spark badly.

4. *Cost of Maintenance.*—This item will be lower, on account of the absence of commutators.

The distributing mains consist chiefly of eight cables of 75 square millemetres section, coupled two and two in parallel.

The following comparisons were made between alternating- and continuous-current motors:—

1. *Price.*—This is about the same for both systems, taking into account the necessary starting apparatus.

2. *Efficiency.*—The results of tests have shown that the efficiency of alternating-current motors is, on an average, about 2 or 3 per cent. higher than for continuous-current motors.

3. *Practical Advantages and Disadvantages.*—These polyphase motors may either have squirrel-cage armatures or armatures designed for inserting a resistance in their circuit.

   Motors above 2 or 3 H.P. with squirrel-cage armatures must be started by means of a transforming device connected to the line itself, in order to diminish the rush of current on starting.

   Under these conditions the speed cannot be varied. The armatures, which are designed for placing resistances in their circuits, are usually wound three-phase, and are fitted with three collector rings, to which

a suitable rheostat is connected. By this means the speed can be varied. Apart, however, from electric traction work, variable speed motors are not in great demand.

4 *The Cost of Maintenance* of rotary-field motors is very low, and consists mainly in refilling the oil reservoirs.

The loss in the line is about 4 per cent., and the efficiency of transmission between the generator and motor pulleys is 0·785, with two alternators working slightly overloaded.

M. VUILLEUMIER—THE ELECTRIC TRAMWAY AT LYONS (CLARET-VUILLEUMIER SYSTEM).

(*Bulletin de la Société Internationale des Electriciens, Vol.* 11, *No.* 113, *p.* 509.)

This tramway was commenced on February 6th, 1894, and was inaugurated on May 2nd of the same year. It runs over a distance of 3,200 metres. A part of it is single track, and a part double track.

A large building, called the "depôt," is used as a shelter for 12 motor cars. In this building are two trenches 20 metres long, to allow of ready inspection of the motor gear.

The motive power in the generating station is obtained from gas engine designed for running with gas of low illuminating power, but with a calorific power of about 1,500 calories. This gas, before being collected in a gasometer, is freed of all dust by causing it to pass through wet coke.

The following are the results of tests made at Lyons:—

Duration of Trial.	Kilowatt-Hours.	Coal Consumed.	Coal per Kilowatt-Hour.
10 hours	607·500	600 kg.	0·9 kg.
10 hours	307·5	400 kg.	1·18 kg.
12 hours	410·4	650 kg.	1·58 kg.
(With 10 cars at work.)			
12 hours	285	600 kg.	2·1 kg.
(Five cars at work, plus the lighting of the depôt.)			

The above consumption of coal includes—

(*a*) The amount of coal consumed during the time the engine is running.

(*b*) The amount of coal burnt during the stoppage of the engine from one day to the next—about 80 kilogrammes.

The gas engine runs at a speed of 125 revolutions per minute, and drives by belt a Thury six-pole dynamo at 300 revolutions and 500 volts. This machine is separately excited at 70 volts, 15 amperes, and the exciter has a special device for maintaining the dynamo potential constant irrespective of the variations of the engine.

Current is conveyed to the motors by an underground conductor, and through distributors fixed at distances of about 100 metres on the track. A number of feeding wires leave these distributors, and are connected up to a corresponding number of insulated contact pieces placed along the track. The approximate length of these contact pieces is about 2 to 3 metres, and they are separated from one another by about the same distances.

These distributors work automatically, and in such a manner that current is switched on to the various sections just as a car is passing over them; current being collected either by two rolling or rubbing contacts fitted one at each end of the car. The current returns to the dynamo by the wheels, rails, and earth. The total width of the track is about 10 metres, and it is of the Marsillon type, with rails and counter rails about 1 metre long. This system can be adapted to any existing track. The results of daily tests give an insulation resistance of about 3,000 ohms. In rainy weather this falls to about 800 ohms. The feeding wires are of copper, with a section of 6 to 7 square millimetres, which is ample for the short time taken by a car to pass over a section. These wires are insulated with vulcanised rubber. The main conductor, which is laid along the whole length of the track, is buried under the pavement at a depth of 0·6 metre, and has an insulation resistance of 1,000 megohms. It has a section of 90 square millimetres; it is lead-sheathed and armoured.

The cost of this system compares well with the overhead trolley system, especially in towns, where the posts carrying the trolley wires must be of iron or steel, and fairly ornamental.

ANON.—THE ELECTRIC CENTRAL STATION OF FRANKFORT.

(*L'Eclairage Electrique. Vol.* 1, 1894, *No.* 12, *p.* 547.)

The contract for the machines was given to Brown, Boveri, & Co., of Baden. The work was commenced in April, and the station was running by November. The station is near the Main and the railway, and covers an area of 2,850 square metres. The machine room is 38 metres long, 23 metres wide, and 10 metres high.

The power is obtained from three tandem engines, each capable of delivering 750 H.P. when running at 85 revolutions per minute. The fly-wheel weighs 24,000 kilogrammes, and the field magnets of the dynamos are built up round its periphery. The engines are fitted with centrifugal governors, and an automatic system of lubrication. They can be worked condensing or non-condensing. The eight boilers have a heating surface of 86 square metres, and work with a Kubon smoke consumer. They work at a pressure of 9 atmospheres. The chimney shaft is 50 metres high. All the steam pipes are placed in an underground gallery. The boilers are fed by two steam pumps, each one being capable of supplying water to all the boilers simultaneously. The station contains an overhead traveller of the full width of the station. It is of 15 tons capacity, and can either be worked by hand or by means of a Brown-Boveri 15-H.P. motor.

The alternators are coupled direct to the engine. The field magnets consist of 54 coils fixed to the fly-wheel, outside which is the stationary armature. The exciter is a six-pole machine with drum-wound armature. The alternators work at a pressure of 3,000 volts. The switch-board is fixed at some height above the floor level, and is of the simplest design. The instruments are of the Hartmann & Braun type.

Six feeders leave the station, and are connected to a high-tension network having 92 transformer stations. The transformers work in oil and are placed in

chambers built under the pavement. The ratio of transformation is 3,000 to 120 volts. The transformers have an efficiency of 96 per cent. at full load, and 94 per cent. at light load. Three sizes are used.

Both high- and low-tension cables are of the Felten & Guillaume concentric type. The section of these cables varies from 25 to 240 square millimetres.

The laying of the cables lasted from July to October. The station is capable of supplying 35,000 incandescent lamps or their equivalent. At present there are connected 10,000 incandescent lamps, 50 arc lamps, and three motors The number of motors will shortly be considerably increased. The cost of the station was estimated at 2,500,000 francs, and the contract was given to Brown, Boveri, & Co. for 2,387,000 francs.

ANON.—THE CALAIS CENTRAL STATION.

(*L'Eclairage Electrique, Vol.* 1, 1894, *No.* 12, *p.* 552.)

The electric lighting of Calais was taken up by the gas company, and the central station was built on their premises, situated at some distance from the centre of distribution. The high-tension alternating-current system with transformers was adopted. The cables are carried on posts fixed in the streets, and the transformers placed in small houses.

The station commenced its supply in June last, and contains two Niel gas engines of 80 H.P. each, having two cylinders, and running at 160 revolutions per minute. The two alternators are by Brown, Boveri, & Co., and have an output of 50 amperes at 1,000 volts. The exciters give 20 to 35 amperes at 80 to 115 volts when running at 1,560 revolutions per minute. The alternators are direct coupled to the gas engines by means of Snyer flexible couplings, consisting of two discs, one of which contains steel brushes which engage in steel teeth on the other disc.

ANON.—THE RESISTANCES OF TRACTION.

(*L'Eclairage Electrique, Vol* 1, 1894, *No.* 13, *p.* 587.)

M. G. Pellisier published an article in *L'Industrie Electrique* on the resistance of traction, with special reference to electric traction. The questions specially considered are—

1. Rolling friction.
2. The additional friction due to curves.
3. Air resistance.
4. The effect due to gradients.
5. The effect of inertia at the moment of starting.

The last point is one of the greatest importance in electric traction.

1. *Rolling Resistance.*—The effort, f, necessary to overcome this resistance can be accurately expressed by $f_1 = \rho$ F. in which F is the weight of the vehicle, and ρ a coefficient depending on the nature of the track. The results of many experiments show that for a clean, wet tramway line $\rho = 6\cdot8$ kilogrammes. For an ordinary line this value becomes 11·5, and for a dusty track 27·5, F being expressed in tons. The mean value of $\rho = 11\cdot5$ kilogrammes may be safely adopted.

The corresponding power, P, is proportional to the speed, v, which is assumed uniform.

$$P_1 = \rho F v.$$

2. *The Friction due to Curves.*—The above formulæ relate to level straight tracks. According to M. Reckenzaun, if the speed be kept constant, the resistance is doubled, and even trebled, when passing over a curve of 15·25 metres radius. As, however, it is habitual to slow up when passing curves, the power required will not be appreciably increased. It would, however, be necessary to consider depreciation of material under these conditions.

3. *Air Resistance.*—This factor, although generally overlooked, may become of considerable importance, especially when the surface of the car is great as compared to its weight. This air resistance is due—

   (*a*) To the motion of the car.

   (*b*) To the wind pressure on the end of the car.

   The first effect may be approximately calculated from the formula,

$$f_2 = a S v^2;$$

   S being the surface in square metres, v the velocity in metres per second. a is a coefficient = 0·08. The corresponding power, P_2, = $a S v^3$.

   The surface, S, of a car is about 7 square metres. The resistance due to wind pressure can be calculated from the same formula by making a = 0·12. It is, however, impossible to make due allowance for variations in its velocity and direction; and this, no doubt, accounts for the variations in the current observed when cars are running on the level and at full speed. These variations sometimes amount to 5 amperes, and, although sometimes attributed to the nature of the track, are more probably due to variations in wind pressure.

4. *Gradients.*—When a car is on an incline the force necessary to overcome rolling friction is slightly diminished; since the pressure normal to the rails is not equal to the weight of the car, but to F cos θ, θ being the angle of the gradient. Under working conditions this difference does not, however, exceed 4 per cent. To overcome the effect due to gravity the motor must develop a force,

$$f_3 = 1{,}000 \text{ F} \sin \theta;$$

   the corresponding power,

$$P_3 = 1{,}000 \text{ F} \sin \theta \, v;$$

   or this formula may be stated in terms of the distance, assuming that it is about equal to the horizontal projection,

$$P_3 = 1{,}000 \text{ F } v \, h,$$

   h being the slope in centimetres per metre. Under these conditions the power necessary to overcome the effect of gravity and rolling friction is

$$P = F (\rho + 1{,}000 \, h) \, v.$$

5. *Starting.*—The power necessary for starting cars becomes of great

importance. Consider a car of weight F attaining its normal speed, v, over a run of l metres. Then—

Mean acceleration ... ... $a = \frac{v^2}{2l}$.

Time taken ... ... $t = \frac{2l}{v}$.

Mean force exerted ... ... $f_4 = 1{,}000 \frac{F}{g} \frac{v^2}{2l}$.

Mean power ... ... $P_4 = 1{,}000 \frac{F}{g} \frac{v^3}{4l}$.

It is assumed above that the acceleration is constant, which is, however, not the case, since the speed can be varied by means of a rheostat.

6. *The total Force and Power required* are made up of the sum of the above factors. It may be assumed that the initial speed is half the maximum speed. The mean force on starting becomes

$$f_a = F(\rho + 1{,}000\, h) + 0{\cdot}08\, S \frac{v^2}{4} + 1{,}000 \frac{F}{g} \frac{v^2}{2l};$$

and the expression for the corresponding power

$$P = F(\rho + 1{,}000\, h) \frac{v}{2} + 0{\cdot}08\, S \frac{v^3}{8} + 1{,}000 \frac{F}{g} \frac{v^3}{4l}.$$

Under normal conditions of running,

$$f = F(\rho + 1{,}000\, h) + 0{\cdot}08\, S\, v^2;$$
$$P_m = F(\rho + 1{,}000\, h)\, v + 0{\cdot}08\, S\, v^3.$$

7. *Applications.*—The author gives a series of tables and curves, calculated from the above formulæ, for quickly solving such problems as the following:—

The total weight of a car weighing 4·5 tons, with 50 passengers, would be about 8 tons. Assume that the car is fitted with two 25-H.P. motors capable of developing 3,200 kilogrammetres per second on the axles. Then—

(1) *What would be the maximum speed attainable on the level with the two motors working at full speed?* If the motors work at full speed, motion is accelerated until the sum of the resistances to motion amounts to 3,200. The result will be about 15 metres per second. It will, however, be necessary to make some allowance for jolting and vibration. From 48 to 50 kilometres per hour may be taken as the maximum speed attainable by a car running under the above conditions. It is interesting to note that at these speeds the air resistance is 50 per cent. greater than rolling friction. The above results have been experimentally confirmed.

(2) *After running what distance, will this speed be attained?* To obtain an approximate figure, it may be assumed that the air and rolling resistance correspond to a mean speed of 24 kilometres per hour. Air resistance would account for 188 kilogrammetres per second, and rolling friction for 640 kilogrammetres per second. There remain 2,370 kilogrammetres per second to overcome inertia; and by the formula

$$P = 1{,}000 \frac{F}{g} \frac{v^3}{4l};\ l = 290 \text{ metres, approximately.}$$

The above speed could only be attained after running a considerable distance.

(3) *What is the maximum speed to adopt in order that the car may start after running over a distance equal to its own length—about* 10 *metres?* As this speed is necessarily low, air resistance may be neglected; and it may be reckoned that 450 kilogrammetres per second are available on the axles. This speed will be about 18 to 19 kilometres per hour, or 12 miles per hour—the same as realised in general practice. At the speed of 20 kilometres per hour the total power absorbed is about 611 kilogrammetres per second, or about one-fifth of the total power of the motors. These conditions would be uneconomical, and in order to load the motors to their full output, and work at highest efficiency, it would be necessary to add about five additional carriages.

All the above figures apply to a straight level track.

J. VIOLLE—ON THE TEMPERATURE OF THE ELECTRIC ARC.

(*Comptes Rendus, Vol.* 119, *No.* 23, *p.* 949.)

When working with currents of 1,000 and 1,200 amperes, the author confirmed the fact, lately published by M. Moissan, that the temperature of the electric arc increases with the current-intensity. On taking photographs of the positive crater, it was found that the brilliancy at 1,000 and 1,200 amperes was the same as at 10 amperes; which tends to show that the crater is the seat of some physical phenomenon, characterised by a constant temperature (boiling point of carbon). The spectra of the crater and of the arc itself were observed, and the luminous intensity was found to differ in the two cases.

An attempt was made to determine the temperature of the arc by introducing into it a thin rod of carbon. When placed in the arc this rod was hollowed out on the positive side and grew on the negative side, the brilliancy of this cavity being about the same as that of the positive crater; the consumption increasing with the current.

A similar rod introduced between two poles of a similar metal is consumed differently according to the metal employed—slowly with copper, rapidly with zinc; moreover, in the latter case the temperature appears to be much above the boiling point of zinc (950°).

In conclusion, the author considers that the temperature of the arc is generally higher than that of the positive carbon, and that the temperature increases with the energy expended.

M. PIERARD—ON THE NATURE OF THE DISTURBANCES PRODUCED IN OVERHEAD TELEPHONE WIRES BY VARIABLE CURRENTS IN NEIGHBOURING CONDUCTORS.

(*L'Eclairage Electrique, Vol.* 1, 1895, *No.* 3, *p.* 121.)

The object of the following experiments was to ascertain to what causes telephonic disturbances are due.

The experiments were carried out on a telephone line situated at a distance of 50 to 300 metres from conductors carrying heavy currents.

First Experiment.—C D is an overhead conductor 3 kilometres long, such as the trolley wire of an electric tramway. A B is a single telephone line influenced by the conductor C D, and situated at a distance of 80 to 100 metres from it. At each end of the line A B is a telephone station, earthed at 5 kilometres from C and about 60 metres from D. A commutating board was so arranged that the latter earth could be conferred several hundred metres away by means of an overhead line perpendicular to the circuit C D. No difference could be noticed on listening in the telephones when contacts were altered from near to distant earth connections.

Second Experiment.—When the contacts were made from distant to near earth connections, a small difference was noted in the noise: it was more intense, and the note higher.

Third Experiment.—The second terminal of the commutating board is in this case connected to a second telephone line, A_1 B_1, similar to the first, and very close to it. At the far end it is connected to earth through a resistance equal to that of the telephone station. The other ends of the lines at B and B_1 are connected together, and insulated from earth. Under these conditions the instruments at B remain silent, but at A act as in the last experiment.

Fourth Experiment.—In this case the connections remain the same at B, but a complete metallic return is substituted for the earth connections at A. In this case the telephones at both ends remain silent.

From the above experiments it may be considered that from a telephonic point of view earth currents have no effect so long as they are not placed too near the rails; the small effect of the earth currents being due to their very low frequency.

Some of the disturbances are due to electro-magnetic action, this effect being proportional to the area of the induced circuit and to the rate of variation of the magnetic field. In the above experiments this area was 3,000 m. × 8 m. = 24,000 sq. m. = S. In experiment 3 the induced surface was very small, being 3,000 m. × 0·25 m = 750 sq. m. = *s*. The electro-magnetic effect was reduced in the ratio of

$$\frac{s}{S} = \frac{750}{24{,}000} = \frac{1}{32},$$

in which case the effect was almost negligible. The last experiments show that some of the disturbance is due to electrostatic effects. The telephones are silent when the two conductors are at equal distances from the disturbing wire and are consequently at the same potential. The author concludes with the following remarks:—

1. The electrostatic effects are those which first come into action, and are noticeable at relatively large distances of 50 to 300 metres, depending on the lengths of the disturbing and disturbed circuits.
2. These effects alone may seriously affect telephonic communication.
3. When one earth connection of the telephone circuit is made very near the rails, then earth currents have some effect.
4 When both earth connections are made near the rails, then the earth

currents have a preponderating effect, the telephone line then acting as a shunt to the return circuit carrying the variable current.

5. The electro-magnetic effects, although not negligible, are less than the above mentioned.

T. TOMASINA—THE ELECTRIC TRAMWAYS OF GENEVA.

(*L'Eclairage Electrique, Vol.* 1, 1895, *No.* 3, *p.* 123.)

This tramway crosses the busiest part of Geneva. Its total length of track is 5.410 metres, with a double-track extension of 1,380 metres. The line is single track, with sidings, 72 per cent. of its length being quite straight, and 75 per cent. of its length being level.

The maximum permissible speed is 12 kilometres per hour in the town itself, and 20 kilometres per hour in the suburbs, with a 15-minute service. Provision has, however, been made for doubling the number of departures per hour.

The Generating Station contains two six-pole self-exciting Thury dynamos, coupled by Raffard flexible couplings to two horizontal Picard turbines, working with a fall of 134 metres. Each dynamo is compound-wound, and has an output of 150 kilowatts at 365 revolutions per minute, working at a pressure of 50 volts.

The main cable is placed underground, and divides into two feeders, which are connected at various points to the overhead line; and at each of these points is a lightning arrester. The ordinary overhead trolley system is employed, with earth return. The overhead line is 7 millimetres diameter; it is of galvanised steel in the town for the sake of safety, and is of copper for the remainder of the circuit. The wire is placed at a height of 6 metres over the centre of the track. The supporting posts are formed of steel tubes.

The Rails are of two types. In the town itself they are of the Marsillon type, laid on transverse wooden sleepers. The weight of this rail is 15·4 kilogrammes per metre, and of the counter rail 11 kilogrammes per metre. The weight of the finished track is 69·2 kilogrammes per metre, excluding sleepers. This type is used over a length of 1 kilometre; the remainder being of the Phœnix type, and weighing 28 kilogrammes per metre.

The rails are laid in lengths of 9 metres; the finished track weighs 82 kilogrammes per metre, including iron sleepers.

The gauge of the rails is 1·445 metres; the maximum slope at any part is 55 millimetres per metre, and the minimum radius of curvature 25 metres. In order to ensure a good metallic return, the rails are bonded with 7-millimetre copper: a copper wire is also laid along the track and connected to the rails at frequent intervals.

Cars.—Eight cars are used, each one being capable of carrying 32 passengers, and an exceptional load of 45 passengers. The total length of the car is 5·85 metres. The length of the carriage itself is 3·8 metres. The width is 2·5 metres.

The truck is supported on flat springs placed between the grease-box and the carcass.

The carriage itself rests on 10 spiral springs. The diameter of the wheels is 0·80 metre, and the distance between the centres of the axles is 2·3 metres.

The brakes consist of four blocks fixed to a strap, and can be controlled from either end of the car. The complete weight of the car without passengers is 4,700 kilogrammes. The car carries one single-reduction motor of 15 to 20 H.P. The armature makes 500 revolutions per minute. The teeth of the spur wheels are machine-tooled with care, in order to ensure smooth and silent running.

The motors are completely enclosed by a steel casting forming the magnetic circuit, thus excluding dust and moisture. The top half is hinged, in order to allow of ready inspection.

The brushes are of carbon. The bearings have automatic ring lubricators.

Each platform is fitted with the necessary controlling apparatus. There is only one handle for use at either end of the car, according to the direction of motion. This handle can only be removed when the switch is on the "Off" contact—that is, when the car is at rest.

The controlling apparatus is used for regulating the speed of the car, or for altering the direction of motion when necessary.

The regulation of speed is effected by means of an external resistance, as this method ensures the smoothest running.

The trolley works on a new principle with sliding contacts, and also allows the car to run at full speed when passing over crossings or curves. The trolley is carried on a hinged wooden pole.

The car is lighted by four 16-candle-power lamps—two in the carriage, and one for each platform.

This line has been in operation since the middle of last September, and has had considerable traffic.

RICARDO ARNO—THE ELECTROSTATIC ROTATION OF RAREFIED GASES.

(*L'Eclairage Electrique, Vol.* 1, 1895, *No.* 3, *p.* 138.)

One of Professor Crookes's experiments on radiant matter consists in causing vanes to rotate by means of electrical discharges, the motion being attributed to a molecular bombardment from the cathode to the anode. M. Arno shows that the vanes can be made to rotate by placing them in a rarefied gas and subjecting them to the action of a rotating electrostatic field.

The method of obtaining the rotating field consists in producing an alternating E.M.F. in the secondary winding of a Ruhmkorff coil, without its interruptor, and working as an ordinary transformer. The secondary circuit is connected up to four vertical metallic sheets forming an incomplete cylindrical surface and placed at 90° to one another. Within the space enclosed by these sheets is placed a glass bulb containing four thin brass vanes rotating round a vertical axis. When the electrostatic field is produced, the vanes will rotate in either direction, according to that of the field.

In these experiments the frequency of the alternating current was 42; the effective E.M.F. of the secondary was 7,500 volts; the diameter of the cylindrical surface formed by the sheets was 15 cm.

Under these conditions the intensity of the rotating field was 1,167 electro-

static C.G.S. units, and the speed of rotation 40 revolutions per minute. The vanes then made 50 revolutions per minute. An ordinary radiometer with mica vanes could not be employed, as a dielectric would rotate under the influence of an alternating field. It was, therefore, indispensable to use metallic vanes. This can be shown under ordinary atmospheric pressure; for where the metallic vanes remained in a position of equilibrium when subjected to the influence of intense rotating fields, the mica vanes under the same conditions were deflected.

In the above experiments the gas in the glass bulb glowed with sufficient brilliancy to allow the speed of the vanes to be counted in the dark.

The author attributes the motion of the vanes to a bombardment of the molecules of the rarefied gas; these molecules moving in the direction of rotation of the field, and under the influence of the field itself.

J. ANIZAN—THE NEW MERCADIER AND ANIZAN MICROPHONE.

(*L'Eclairage Electrique, Vol.* 1, 1894, *No.* 15, *p.* 677.)

The intensity and sharpness of sound produced by a microphone depends essentially on the nature of the microphonic contacts. Although the intensity of sound with certain types of microphone may be great enough, the sharpness falls off beyond certain distances; and the vowels e, u, i, and consonants f, c, v, s, z, are so badly reproduced as to render the words indistinguishable.

Microphones can be roughly divided into two classes—those working with granulated carbon or filaments of carbon, and those working with carbon pencils. The former, although working fairly satisfactorily, fail beyond a certain point, owing to the crowding up of the carbon granules. The latter, although yielding less brilliant results, are more constant in their action: for this reason this type is the one most in favour.

The microphone in question consists of two carbon rods and eight carbon pencils. The main fault with carbon pencils lies in the instability of the contact. The carbons of the Mercadier-Anizan microphone are of cylindrical form, fitted tightly at one end of a brass rod, which has a conical cavity at the other end, into which fits a metallic point used as a support for the carbon.

The two carbon rods consist of half-cylinders, round which V grooves are cut at right angles to the axis of the rod. These two rods are fixed to a circular diaphragm placed in a vertical position. These rods are fixed horizontally, and insulated from one another. The eight carbon pencils described above rest in the V grooves, two stable contacts being thus obtained with each pencil. The pressure at each contact can be varied by altering the slope of the pencil. The microphone is so connected up as to offer four series of four contacts. The electrical resistance of the contacts when at rest is 7 to 8 ohms. With an E.M.F. of 3 volts and low-resistance cells the current in circuit is 0·3 ampere. Very clear speaking was obtained with this, working through an artificial line consisting of resistance and capacity.

With the pencils adjusted to their minimum slope good results were obtained through an artificial line, equivalent to 400 kilometres of iron wire with an ohmic

resistance of 10 ohms per kilometre and a capacity of 0·007 microfarad per kilometre.

This microphone works equally well for short distances when fitted with the special adapter described in *La Lumière Electrique*, 7th April, 1894.

R. COLSON—ON THE MEASUREMENT OF RESISTANCE BY MEANS OF ALTERNATING CURRENTS AND THE TELEPHONE.

(*Comptes Rendus, Vol.* 119, *No.* 27, *p.* 1261.)

The usual method of employing the telephone for measuring resistances consists in sending periodic currents through a Wheatstone bridge and so balancing the arms that no sound is heard in the telephone.

The conductors forming the arms must be free of self-induction and of polarisation.

The author makes the following conditional remarks:—

1. This method depends on Ohm's law, so long as the waves employed are of high potential, such as those obtained from an induction coil, and working through high resistances.
2. The potential at each of the points C and D, across which the telephone is connected, is the resultant of two waves of opposite sign coming in opposite directions. It may happen that the waves take place at such a time at C and D as to produce a sound in the telephone of the same frequency or double the frequency of the source. It is therefore necessary to provide against either of these occurrences. The conditions at C and D at any moment should be identical, and the resultant potential should be zero: no sound will then be heard in the telephone.

It may also be necessary to provide against disturbances in the other arms of the bridge, due to the varying potential at C or D, by adjusting the working potential or the resistances of the arms.

Without taking these precautions the above method is subject to considerable errors.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the month of FEBRUARY, 1895.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHTING AND POWER.

R. V. Picou—Transmission of Force by Synchronous Alternate-Current Motors.—*Bull. Soc. Int.*. vol. 12, No. 115, p. 60.

J. Reyval—Experiments made on Boilers at the Frankfort Exhibition.—*Ecl. El.*, vol. 1, No, 5, p. 207.

O. Koehler—Electric Motors in a Sugar Factory.—*Ibid.*, p. 218.

A. Monmerqué—Electric Light Main Conduits in Paris.—*Ecl. El.*. vol. 1, No. 6, p. 253, No. 7, p. 304 (S. I.).

A. H.—The Chemnitz Central Station.—*Ibid.*, p. 263.

F. Jordan—The Bremen Municipal Electric Light Works.—*E. T. Z.*, 1895, No. 6, p. 75 (I.).

L. Fischer—The Design of Polyphase Plant and Installations.—*Ibid.*, p. 80, No. 7, p. 100 (I.).

T. Stort—New Alternate-Current Arc Lamp of the Elektricitäts-Aktien-Gesellschaft, formerly Schuckert & Co.—*E. T. Z.*, 1895, No. 9, p. 124 (I.).

W. Weissenbach-Griffin—Dowson Gas Engine for Electrical Working in Switzerland.—*Ibid.*, p. 129.

DYNAMO AND MOTOR DESIGN.

A. Hess—On the Slow Increase of the Loss of Energy in Transformers.—*Ecl. El.*, vol. 1, No. 7, p. 307.

L. Legrand—Theory and Design of Non-Synchronous Motors with Rotary Magnetic Fields.—*Ecl. El.*, vol. 1, No. 8, p. 341 (I.).

— Farman—A few Words on the Design of Dynamo Machines.—*Ibid.*, 348 (I.)

V. Dolivo-Dobrowolsky—Machines for Simple and Polyphase Alternating Currents made by the Allgemeine Elektricitäts-Gesellschaft of Berlin.—*Ibid.*, p. 364; *E. T. Z.*, 1895, No. 6, p. 95 (I.).

Anon.—The Increase of the Open-Circuit Loss in Transformers.—*E. T. Z.*, 1895, No. 6, p. 85.

Anon.—The Wood Arc Lighter.—*E. T. Z.*, 1895, No. 7, p. 101 (I.).

O. S. Bragstad—Experiments on a Rotary Field.—*E. T. Z.*, 1895, No. 8, p. 112 (I.).

ELECTRIC TRACTION.

Anon.—Electric Traction of a Carriage by means of Tommassi Accumulators.—*Jour. Tel.*, vol. 19, No. 2, p. 41.

H. S. Wynkoop—The Supplementary Return Conductor for Electric Tramways. —*Ecl. El.*, vol. 1, No. 5, p. 220.

J. Rigge—Analysis of Electrolysed Pipes.—*Ecl. El.*, vol. 1, No. 6, p. 259.

Anon.—Electric Tramways with Underground Conductor.—*Ecl. El.*, vol. 1, No. 8, p. 363.

Anon.—Accumulator Cars for Hanover.—*E. T. Z.*, 1895, No. 6, p. 89.

A. Paupié—Development and Working of Tramways in Hungary.—*Ibid.*, p. 89.

Anon.—The Projected Elevated Electric Railway in Berlin.—*E. T. Z.*, 1895, No. 7, p. 103.

Anon.—Accumulator Traction in Berlin.—*Ibid.*, No. 8, p. 119, No. 9, p. 130.

Anon.—Electric Traction in Rome.—*Ibid.*, No. 9, p. 131.

STATIC AND ATMOSPHERIC ELECTRICITY.

Sir D. Salomons—On certain Phenomena observed in Vacuum Tubes.—*Ecl. El.*, vol. 1, No. 7, p. 322.

A. Righi—On Electric Oscillations of Short Wave-Length, and their Employment in the Production of Phenomena analogous to the principal Phenomena of Optics.—*Ecl. El.*, vol. 1, No. 8, p. 350 (I.).

Bedell and Kinsley—Study of Residual Charges in Condensers, and the Influence of Temperature on them.—*Ibid.*, p. 374 (I.).

F. Womack—A Modification of the Ballistic Galvanometer Method of Determining the Electro-magnetic Capacity of a Condenser.—*Phil. Mag.*, vol. 39, No. 237, p. 172

A. Herz—On the Question of the Potential Gradients in the Positive Portion of the Brush Discharge.—*W. A.*, vol. 54, No. 2, p. 244 (I.)

E. Warburg—On Conduction of Heat and Temperature of the Glowing Gases in Geissler Tubes.—*Ibid.*, p. 265.

K. Mack—Double Refraction of Electric Radiation.—*Ibid.*, p. 342.

P. Drude—Researches on Electric Dispersion.—*Ibid.*, p. 352.

E. Goldstein—The Action of Kathode Rays on certain Salts.—*Ibid.*, p. 371.

E. Aschkinass—On the Influence of Electric Waves on the Galvanic Resistance of Metallic Conductors.—*W. A.*, vol. 54, No. 2, p. 103, suppl.

C. B. Thwing—A Relation between the Specific Inductive Capacity and the Chemical Constitution of the Dielectric.—*Beibl.*, vol. 19, No. 2. p. 184.

C. Rovelli—On the Side Discharges produced by Electric Currents of High Frequency or of very Rapidly Varying Intensity.—*Ibid.*, p. 199.

J. Trowbridge—Alteration of the Period of Electric Waves along Iron Wires.—*Ibid.*, p. 199.

V. Dvorak—Remarks on the Theory of Atmospheric Electricity.—*Ibid.*, p. 208.
Elster and Geitel—Electrical Observations on the Sonnblick.—*Ibid.*, p. 208.

MAGNETISM.

D. Hurmuzescu—Magnetism and Chemical Reactions. The Electro-motive Force of Magnetisation.—*Ecl. El.*, vol. 1, No. 6, p. 248, No. 7, p. 297 (I.).
T. Gray—On the Measurement of the Magnetic Properties of Iron.—*Ibid.*, p. 279.
H. Tomlinson—Effect of Mechanical Forces and of Magnetisation on the Physical Properties of Alloys of Iron, Nickel, and Steel with Manganese.—*Ecl. El.*, vol. 1, No. 7, p. 321.
S. Bidwell—Influence of Magnetisation on the Dimensions of Rings of Iron in Directions Perpendicular to the Lines of Force, and on the Volumes of the Rings.—*Ibid.*, p. 324 (I.).
J. Hopkinson—Influence of Internal Electric Currents on the Propagation of the Magnetisation of Iron.—*Ibid.*, p. 326.
Thompson and Walker—Mirrors of Magnetism.—*Phil. Mag.*, vol. 39, No. 237, p. 2 3.
B. Rosing—On the Change of Length in Soft Iron Wire placed in a Uniform Magnetic Field.—*Phil. Mag.*, vol. 39, No. 237, p. 226 (I.).
E. Lecher—A Study of Unipolar Induction —*W. A.*, vol. 54, No. 2, p. 276 (I.).
M. Ascoli—On Magnetic Shielding.—*Ibid.*, p. 381.
M. Ascoli and F. Lori—On the Distribution of Induced Magnetism in Iron, and on the Demagnetising Factor in Iron Cylinders.—*Ibid.*, p. 196.
A. Schmidt—On the Results up to Date and the Future Problems of the Researches on Terrestrial Magnetism.—*Ibid.*, p. 209.
J. Liznaar—A Contribution to the Knowledge of the 26-Day Period of the Earth's Magnetism.—*Ibid.*, p. 210.
G. Folgheraiter—Direction and Intensity of the Permanent Magnetism of the Volcanic Rocks of Latium.—*Ibid.*, p. 210.

ACCUMULATORS.

G. H. Condict—Employment of Accumulators in an Electric Light and Traction Central Power Station.—*Ecl. El.*, vol. 1. No. 7, p. 311.
Elbs and Schoenherr—On the Chemical Reactions in Lead Accumulators.—*Ecl. El.*, vol. 1, No. 8, p. 368.
H K.—The Chloride Accumulator.—*E. T. Z.*, 1895, No. 6, p. 86.

TELEGRAPHY AND TELEPHONY.

A. Tobler—On the Figures of Merit of certain Telephone Station Calls.—*Jour. Tel.*, vol. 19, No. 2, p. 25 (I.).
Anon.—Telegraphs and Telephones in Germany in 1893.—*Ibid.*, p. 28

ANON.—Telegraphs and Telephones in the Scandinavian States at the End of the Year 1893.—*Ibid.*, p. 32.

ANON.—Federal Law of 7th December, 1894, dealing with the Reduction of Telephone Tariffs in Switzerland.—*Ibid.*, p. 36.

ANON.—Report of the Western Union Telegraph Co. for the Year ending 30th June, 1894.—*Ibid.*, p. 38.

G. DE LA TOUANNE—Notes on Telephony in the United States.—*Ecl. El.*, vol. 1, No. 7, p. 313, No. 8, p. 369 (I.).

ANON.—Experiments in Telephonic Communication with Bare Wires laid on the Ground.—*E. T. Z.*, 1895, No. 6, p. 83 (I.).

ANON.—The Telegraph Cables of the World.—*Ibid.*, p. 87.

ANON.—Telegraphy without Wires.—*Ibid.*, p. 88.

ANON.—Use of Railway Lines for the Conduction of Telegraphic and Signalling Currents.—*E. T. Z.*, 1895, p. 7, p. 102.

W. FINN—Accumulators in American Telegraph Working.—*E. T. Z.*, 1895, No. 8, p. 125 (I.).

INSTRUMENTS AND MEASUREMENTS.

E. VAN AUBEL—On the Electric Resistance of certain New Alloys.—*Jour. de Phys.*, February, 1895, p. 72.

J. VOISENAT—Elisha Gray's Telautograph.—*Bull. Soc. Int.*, vol. 12, No. 115, p. 72 (I.).

J. CAURO—On the Electrostatic Capacity of Bobbins, and its Influence on the Measurement of Self-Induction Coefficients by the Wheatstone Bridge Method. —*Ibid.*, p. 96; *C. R.*, vol. 120, No. 6, p. 308.

C. BRÜGGEMANN—Measurements made on the Gülcher Thermo-electric Batteries. —*Ecl. El.*, vol. 1, No. 6, p. 260 (I.).

H. ABRAHAM—Alternating Currents of High Frequency and the Wheatstone Bridge.—*Ibid.*, p. 270.

A. RIGHI—A New very Sensitive Idiostatic Electrometer.—*Ibid.*, p. 275 (I.).

SWAN and RHODIN—Measurement of Absolute Specific Resistance of Electrolytic Copper.—*Ibid.*, p. 280.

R. ARNO—On the Employment of the Quadrant Electrometer as a Differential Instrument.—*Ecl. El.*, vol. 1, No. 7, p. 327 (I.).

F. HIMSTEDT—On an Absolute Measurement of Resistance.—*W. A.*, vol. 54, No. 2, p. 305 (I.).

F. HIMSTEDT—On the Determination of the Self-Induction of Coils of Wire.—*Ibid.*, p. 335.

— MCKITTRICK—On the Measurement of the Resistance of Electrolytes.—*Beibl.*, vol. 19, No. 2, p. 186.

R. O. PEIRCE—On the Thermo-electric Properties of Platinoid and Manganin.—*Ibid.*, p. 189.

T. BRUGER—On Direct-Reading Measuring Instruments.—*Ibid.*, p. 193.

ANON.—Fiske's Electric Range-Finder.—*E. T. Z.*, 1895, No. 7, p. 104.

H. SESEMANN—Protection of Small-Current Circuits against Strong Currents.—*E. T. Z.*, 1895, No. 8, p. 115 (I.).

ELECTRO-CHEMISTRY.

L. Cailletet and Colardeau—Researches on the Condensation of the Gases formed by Electrolysis, by Porous Bodies, and in particular by Metals of the Platinum Family; Application to Gas Batteries. High-Pressure Electric Accumulators.—*Jour. de Phys.*, February, 1895, p. 62.

J. Garnier—Action of an Electric Current on a Series of Metallic Sulphides in a Molten Condition.—*Bull. Soc. Int.*, vol. 12, No. 115, p. 95.

P. H. Ledeboer—First Annual Meeting of the German Electro-chemical Society at Berlin.—*Ecl. El.*, vol. 1, No. 5, p. 209.

P. Schoop—Circulating Arrangements for Electrolytic Apparatus.—*Ecl. El.*, vol. 1, No. 5, p. 221 (I.).

Anon.—On Electric Primary Batteries.—*Ibid.*, p. 225.

D. Hurmuzescu—Magnetism and Chemical Reactions.—*Ecl. El.*, vol. 1, No. 6, p. 248, No. 7, p. 297 (I.).

N. Strindberg—On the Alteration of the Conductivity of a Solution by Addition of Small Quantities of a Non-Conductor.—*Beibl.*, vol. 19, No. 2, p. 186.

V. Rothmund—The Potential Differences between Metals and Electrolytes.—*Ibid.*, p. 187.

J. W. Swan—On some Voltaic Cells with Molten Electrolytes and Gaseous Depolarising Substances.—*Ibid.*, p. 188.

H. Bagard—On the Thermo-electric Forces between Two Electrolytes, and the Electric Transmission of Heat in Electrolytes.—*Ibid.*, p. 190.

THEORY.

— Vaschy—On the Nature of the Maxwell Displacement Current.—*Bull. Soc. Int.*, vol. 12, No. 115, p. 95; *C. R.*, vol. 120, No. 5, p. 255.

H. Pellat—Electrostatics not founded on the Laws of Coulomb: The Electric Force acting at the Surface of Separation of Two Dielectrics.—*Ecl. El.*, vol. 1, No. 7, p. 289.

L. Lombardi—Variation of Potential and of Intensity of Current in an Open Circuit moving in a Uniform Magnetic Field.—*Ibid.*, p. 329.

L. Boltzmann—On the Newest Theories of Electricity and Magnetism.—*Beibl.*, vol. 19, No. 2, p. 201.

L. Fischer—The Theory and Design of Polyphase Plant and Installations.—*E. T. Z.*, 1895, No. 6, p. 80 (I.), No. 7, p. 100.

VARIOUS.

A. Sokolow—On the Tension of Saturated Vapour in an Electric Field.—*Jour. de Phys.*, February, 1895, p. 53.

D. Farman—On Disinfection by Electricity: Hermite System.—*Bull. Soc. Int.*, vol. 12, No. 115, p. 51 (I.).

F. P.—Electric Projectors, and their Use in War.—*Ecl. El.*, vol. 1, No. 5, p. 193 (I.).

ANON.—The Effects of Lightning on Workshop Chimneys.—*Ibid.*, p. 214.

ANON.—Electricity and the Development of Plants.—*Ibid.*, p. 215.

ANON.—An Electric Vignetter.—*Ibid.*, p. 218.

CLAYTON SHARP and TURNBULL—Bolometric Investigation of Photometric Scales.—*Ibid*, p. 233 (I.).

P. JANET—On an Electro-chemical Method of Drawing the Curves of an Alternating Current.—*Ecl. El.*, vol. 1, No. 6, p. 241 (I.).

P. MARCILLAC—Electric War Signalling.—*Ibid.*, p. 255 (I.).

B. F. MILES—The Manufacture of Arc Lamp Carbons in America.—*Ibid.*, p. 265 (I.).

THRELFALL, BREARLEY, and ALLEN—On the Electric Properties of Pure Sulphur.—*Ibid.*, p. 278.

J. BLONDIN—McCrehore's Method of Drawing the Curves of a Variable Current.—*Ecl. El.*, vol. 1, No. 8, p. 337 (I.).

AYRTON and HAYCRAFT—Students' Simple Apparatus for Determining the Mechanical Equivalent of Heat.—*Phil. Mag.*, vol. 39, No. 237, p. 160 (I.).

A. SCHUSTER—Electrical Notes.—*Ibid.*, p. 175.

C. H. WIND—On some Novel Researches relating to the Kerr Phenomenon.—*W. A.*, vol. 54, No. 2, p. 84, suppl.; *Beibl.*, vol. 19, No. 2, p. 197.

O. FRÖLICH—On the Practical Applications of Ozone.—*Ibid.*, p. 205.

ANON.—The German Electrical Industry in 1894.—*E. T. Z.*, 1895, No. 6, p. 86.

ANON.—Safety Arrangements for Protection of Electrical Installations against Fire.—*E. T. Z.*, 1895, No. 7, p. 104, No. 9, p. 126.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXIV. 1895. No. 117.

The Two Hundred and Seventy-fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 14th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on February 28th, 1895, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

George Henry Cottam.	The Earl Russell.

From the class of Students to that of Associates—

Willoughby Lake Baylay.	Henry Cooke Leake.
Arnold Hartley.	David T. Powell.
William Thomas Hodgson.	

Donations to the Library were announced as having been received since the last meeting from Dr. John Hopkinson, and E. Wilson and C. S. James, Members.

Mr. Faulkenstein and Mr. Miller were appointed scrutineers of the ballot.

The PRESIDENT: I have now to call upon Mr. Keith to read his paper on "The Electrolysis of Gold."

THE ELECTROLYSIS OF GOLD.*

By N. S. KEITH, Member.

Dr Keith. Obtaining the contents of gold from auriferous rocks and ores by means of solvents and subsequent precipitation is now attracting much attention, in both scientific and financial circles.

Because in some of the practised plans electricity takes an important part in the operations, I think some electrical information relating to the matter will be acceptable; therefore this paper.

In fully considering the subject, we would have to enter into the question of the possible formation of native gold in rocks and ores by electro-deposition from the solvent waters of the earth by action of earth currents. But, as my paper is to deal more with practice than hypothesis, I shall leave that matter to the side, after stating a fact from which various deductions may be drawn. At the Dorn Gold Mine, in South Carolina, U.S.A., the rock formation is talcose slate. The soil is mainly the slate, disintegrated and decomposed. The slate is permeated by quartz veins, of all sizes, carrying gold. The soil is also auriferous. Particles of gold in the soil are all rounded, as if they had been submitted to attrition or solvent action; while those in the rock are crystalline in form, bright, and with sharp, well-defined edges. The gold from both positions is of equal quality, and the soil is of local origin. These facts seem to point to the present decrease of gold from the soil by solution, and increase, by deposition, of the gold in the rock.

Gold exists in natural auriferous materials in its native, or metallic, state, in irregularly shaped pieces, from scarcely above the size of an atom to that of, say, a hundredweight. The larger pieces give no special trouble in securing, after they are found, and do not call for our consideration in this paper. The smaller particles—those of which it takes thousands, perhaps, to weigh a milligramme—are the minute things which engage our present

* See note on page 275.

attention, and the efforts of metallurgists and electro-chemists the world over. Dr. Keith.

The larger particles of gold are comparatively easily separated by amalgamation from the powdered auriferous rock, owing to their weight causing them to sink into contact with mercury surfaces. But the lighter particles float in the water used, do not come into contact with mercury, and are lost to amalgamation. Many efforts have been made to use electricity in obtaining this finely divided gold. Most of them have been the dreams of unlearned enthusiasts, or the devices of dishonest men to obtain gold from the pockets of their dupes.

The "Rae process," which has received so much consideration at the hands of "experts" in recent patent litigation involving the "cyanide process" of McArthur-Forrest, was an effort to simultaneously dissolve gold from ores and deposit it on a cathode by electrolytic action. It failed for at least two reasons—(1) the imperfect knowledge of electro-chemistry of its inventor; and (2) from the impractibility of causing the particles of gold to remain long enough in contact with the anode to be acted on electrolytically. The dynamos used by Rae at the Douglas Mill, in Nevada, about eight years ago, in the effort to gain practical results, were series-wound, and without polarity protectors. Carbon anodes (sometimes, by reversal, the cathodes) were used, suspended in amalgamating pans, in which the pulp was agitated. The sides and bottoms of the iron pans were to be the cathodes. At least half the time they were the anodes. The mercury in the pulp was to combine with the precipitated gold and silver.

By other inventors agitated mercury was made the cathode, and the fine gold was expected to mechanically "follow the "current" into the mercury.

Whatever of value the electric current conferred was due to the fact that there was a reduction at the cathode of oxides and salts of metals, thus keeping the mercury with a clean, bright, unoxidised surface, so that it was in a more receptive condition.

The amounts of various metals dissolved or deposited by a coulomb of current have been accurately determined. For example, one coulomb sets free 0·000162 grain of hydrogen. We

Dr. Keith. may express this quantity as one hydrogen-coulomb = 0·000162 grain. From this we gain the following values:—

One (dyad) copper-coulomb $= \frac{0·000162 \times 63·5}{2} = 0·0051035$ grain of copper.

One (monad) silver-coulomb $= 0·000162 \times 108 = 0·017343$ grain of silver.

One (triad) gold-coulomb $= \frac{0·000162 \times 196·6}{3} = 0·010611$ grain of gold.

One (dyad) mercury-coulomb $= \frac{0·000162 \times 200}{2} = 0·016200$ grain of mercury.

One (dyad) iron-coulomb $= \frac{0·000162 \times 56}{2} = 0·004536$ grain of iron.

One (dyad) lead-coulomb $= \frac{0·000162 \times 207}{2} = 0·016767$ grain of lead.

One (dyad) zinc-coulomb $\frac{0·000162 \times 65}{2} = 0·005265$ grain of zinc.

For a practical, everyday-work unit, we may multiply each of the above quantities by 3,600 seconds in an hour to express the several quantities which an ampere will cause the solution and deposition of each hour. These quantities are as follows:—

Hydrogen-ampere-hour ...			...	0·5832	grain.
Copper	„	...	...	18·37	grains.
Silver	„	...	...	72·28	„
Gold	„	...	...	38·19	„
Mercury	„	...	...	58·32	„
Iron	„	...	...	16·30	„
Lead	„	...	...	60·36	„
Zinc	„	...	...	18·95	„

It has been found by experimenters in the line, and it may be accepted as a law, that the various elements bear a relation to each other, which relation we name "electro-positive" or "electro-"negative," according to the method of expression. Some call it their "electro-chemical" relation. The electro-positive metals tend to composition with electro-negative elements. Thus, potassium is always electro-positive to the electro-negative oxygen. Elaborate lists of the electro-positive and the electro-negative elements are in the text-books. They have the order of arrangement which puts the most electro-positive first, followed, *seriatim*, by the less electro-positive, till at the lower end the most electro-negative element appears. But one element may be electro-positive to another element in one solution, while in another solution the

electro-chemical relation is exactly the reverse. For instance, in dilute sulphuric acid iron is electro-positive to gold; but in a solution of cyanide of potassium gold is electro-positive to iron. The electro-positive is always more soluble in the solution than is the electro-negative. If the electro-positive and the electro-negative metals be connected, electrically, while in the solution, the solving action of the liquid on the electro-positive metal is much increased—somewhat in proportion to the difference in their electro-chemical relation. Such a combination makes a galvanic battery, more or less approaching perfection. The placing of an extraneous source of electricity in such a circuit may increase the action, or even reverse it, depending on the direction of the electric current. The dynamic value of these actions may always be measured as watts by suitable instruments, such as an ammeter and a voltmeter. Dr. Keith.

The facts that gold could be dissolved by chlorine, aqua regia, bromine, cyanide of potassium, &c., have been known for many years. These facts have been put extensively into practical use for the obtaining of gold from ores by means of chlorine gas, in the Plattner process, and its modifications. From the solutions so obtained the gold is deposited by ferrous sulphate ($Fe\ S\ O_4$). For one reason or another, not necessary to consider here, electrolytic precipitation of the gold from this solution of auric chloride ($Au\ Cl_3$) has not been practised in the art. Electro-gilding, using gold anodes, and various metallic objects as cathodes, in an electrolyte consisting of auro-cyanide of potassium (K Cy Au Cy) in a solution of potassic cyanide (K Cy) has been practised for about 50 years. Its details are too well known to need consideration here.

We will consider the subject in its bearing upon the obtaining of gold from auriferous rocks and ores. For this purpose the most important solvent in practical use at present is cyanide of potassium, measured by the production of gold from the mines by its means. It has been stoutly asserted that cyanide of potassium is *not* a solvent of gold, and more surely maintained that it is, by reason of its extensive use for the purpose, spite of the fact that pure solution of potassic cyanide in water will not dissolve

Dr. Keith. gold at a practicable rate, if at all. If, for an experiment, we take a piece of gold leaf and lay it on the surface of a cyanide solution, we will find that it will there be dissolved in a few minutes (the stronger the solution the more rapidly), while a like piece sunk in the solution will be hours in dissolving, the strength of the solution affecting the rate but slightly. It is claimed, and justly too, that the presence of oxygen is necessary for the hastening of the solving action, or, rather, the combination of cyanogen with the gold, so that the resulting auric cyanide be dissolved. This fact was recognised upwards of 50 years ago by Elsner, and the chemical equation was expressed by him thus:

$$4\,Au + 8\,K\,Cy + O_2 + 2\,H_2\,O = 4\,Au\,Cy\,K\,Cy + 4\,K\,O\,H;$$

the oxygen in this case furnishing the necessary element to decompose some of the potassic cyanide so as to form the double salt, auric-potassic cyanide and potassic hydrate.

MacLaurin, a New Zealand chemist, has proved with mathematical precision the truth of Elsner's equation. His admirable research was published more than a year ago in the *Journal of the Chemical Society* (London), 1893, p. 724.

Gold leaf of the same thickness and very nearly the same weight was allowed to remain—

	In stoppered bottle.		In open bottle.		In bottle with O.	
	92	hrs.	68	hrs.	96	hrs.
Water	125	grm.	125	grm.	125	grm.
Potassium cyanide	1	grm.	1	grm.	1	grm.
Gold	0·9947	grm.	0·99035	grm.	0·99425	grm.
Loss of gold	0·0018	grm. = 0·18%	0·09175	grm. = 9·1%	0·23995	grm. = 24·2%

The minute quantity of gold dissolved in the stoppered bottle is apparently due to a small quantity of oxygen previously absorbed by the cyanide solution from the atmosphere. Just in the same way, water, though not a solvent for calcium carbonate, will dissolve the latter to some extent if not previously freed from carbonic acid by boiling.

Recognising the apparent necessity of the presence of oxygen, many experimenters have tried many oxidising substances, besides passing air through the cyanide solution, such as peroxide of manganese; ferricyanide of potassium; bleaching powder, or

other materials yielding chlorine, bromine, and iodine; peroxide of sodium; I have tried peroxide of hydrogen with success. All these are more or less effectual in hastening the solvent action; but, as they have to be added in quantities far in excess of the amount theoretically necessary to effect the reactions, if actually brought into contact with the gold and nothing else, they are wasteful, because they decompose more of the cyanide than is necessary for solvent action. Bromine, chlorine, &c., are simply oxidising agents—secondarily, to be sure, but still decomposers of the cyanide in too great quantities, because they exert their action away from the particles of gold. These oxidising agents have been tersely called "cyanicides." Their actions, direct or otherwise, are practically those of oxygen. Dr. Keith.

Be that as it may; in practice the finely divided auriferous rock, or ores, is submitted to the action of successive quantities of aqueous solutions of cyanide of potassium, containing varying quantities of oxygen, or oxygen-producing materials, or analogues, in large vats, as in infiltration and percolation. Each quantity of the solvent is allowed to remain for a greater or less number of hours in, and covering, the ore, and is then drawn therefrom into precipitation vats, where the gold is obtained from the solution by various means, which we will presently consider. Generally, the finely divided auriferous materials are "tailings" from the gold amalgamating mills, which crush and treat with mercury the material for obtaining the contents of gold by amalgamation, as far as possible.

These tailings contain extremely fine-divided gold which has resisted amalgamation, and in varying quantities, averaging about 5 to 6 pennyweights per ton of tailings. Any economy in the above process, such as hastening the rate of solving the gold; prevention of decomposition, as far as possible and feasible, of cyanide; making the gold more easily and cheaply obtainable from the solution, &c., is of vast importance in the metallurgy of gold.

Looking at the matter from the electro-metallurgist's standpoint, some time ago I concluded that the action of oxygen was due to its strong electro-negative relation to gold in a cyanide

Dr Keith. solution. Therefore I tried the effects of various materials which are electro-negative to gold in solutions of cyanide of potassium. Carbon in contact with gold in cyanide solutions forms with it a voltaic couple, and the gold is far more rapidly dissolved than when alone. The best practical application of this fact is to finely powder the carbon, and continually agitate it with the auriferous tailings in a cyanide solution. Though the finely powdered carbon causes more speedy action than do a few, or more, large pieces, it is still an operation of chance, because the increased rate of action is dependent upon actual continued contact of gold and carbon until the gold is dissolved. Iron is electro-negative to gold in a cyanide solution. The two form a galvanic or voltaic couple, and the action is the same as in the case of the carbon, only less in speed, because the iron is not so strongly electro-negative as is the carbon. I have discovered that mercury is electro-negative to gold in a solution of cyanide of potassium. I have availed myself of this fact to improve the cyanide solvent, after this manner: I add to the cyanide of potassium solution used for the purpose, in any of its strengths (which latter vary from 0·01 per cent. to 0·5 per cent. of cyanide), a small quantity of potassio-mercuric cyanide ($2\ K\ Cy\ Hg\ Cy_2$), say two ounces, or more, per ton of solution. The action is as follows:—The electro-positive gold decomposes the mercuric cyanide, taking to itself the cyanogen thereof, and receives on its surface the liberated mercury which amalgamates it. Then in the solvent there is the contact of a voltaic couple, under which condition the electro-positive gold is dissolved, and afterwards the extremely attenuated film of mercury again dissolves in the cyanide solution, to react as before. There are no combinations, decompositions, and reactions taking place, except those noted as being caused by the voltaic couple.

There are no oxidations of cyanides to cyanates, which latter are not solvents of gold. The solving action takes place very much speedier, because the electro-negative material is brought into, and retained till dissolved in direct contact with, the electro-positive gold. There is no deleterious material introduced to the solvent, and the mercury acts as an assistant in the subsequent

operations of obtaining the gold from the solvent. In the older process, two, three, or more, changes of the solvent were made, with intervals between, during which the oxygen of the air was permitted to permeate the mass of tailings, and thus furnish the necessary electro-negative material. By my electrolytic process, the necessary electro-negative material, mercury, is always present; so that the solution may go on uninterruptedly so long as the necessary solvent, cyanide of potassium, is present in contact with the couple. But, in order to ensure this condition, especially with extremely weak solutions of cyanide, the use of which is desirable, the solvent must be kept in motion through the mass, so that the part containing the cyanogen constituent which should be in contact with the gold may continuously replace the solution which has acquired its *quantum* of gold. Dr. Keith.

Pursuing the above plan, extremely weak solutions of cyanide of potassium may be used—say 0·01 per centum, or less—by percolating them through the mass of tailings continuously, and simultaneously passing the solvent from the percolator through an electrolytic bath with sufficiently extended electrode surfaces, and enough amperes of current to ensure the electro-deposition of the dissolved gold from the solvent wholly, or in greater part. The solvent can be returned continuously to the tailings to dissolve more gold, if necessary; or, if practically all the gold has been removed, it may be passed to a fresh lot of tailings, to again be passed through the cycle of operations.

The most extensively pursued plan of obtaining the gold from auriferous cyanide solutions is by precipitation by means of zinc. Practice has demonstrated that for effective deposition the zinc must be finely divided, and its surfaces must be bright and metallic, free from oxide or salts. Therefore the "zinc "shavings" are always freshly prepared before the time of use.

They are put in boxes having bottoms of iron-wire netting, and these boxes are so placed in the precipitation vat that the solution enters each box of the series through the bottom, up through the mass of zinc shavings, down again, and up through the next box of the series. Having passed through the last box, much of the gold will have been deposited by auto-electrolytic

Dr. Keith. action; and the solution, after being strengthened by the addition of more cyanide, is used to dissolve more gold, as before.

The electro-chemical action is as follows:—The electro-positive zinc takes the cyanogen from the relatively electro-negative gold to form zincic cyanide, which dissolves in the solution, setting free the gold in a finely divided metallic condition. If the action would stop there this process would not be so objectionable. But owing to the voltaic couples formed by the zinc and gold, there is a further decomposition of the solution, with the formation of oxy-cyanate of zinc, zincate of potash, potassic hydrate, &c. Much of the deposited gold, becoming separated from electric contact with zinc, is re-dissolved by the cyanide, to be again deposited by more zinc. So it is practically impossible to precipitate all the gold by this means.

If the necessary actions could take place in true electro-chemical equivalents, according to our table, 31·5 troy ounces of zinc would only be requisite to deposit 65·55 ounces of gold. Instead of that, the practical amount, owing to the above-described wastes, and others, is about in the proportion of 1,750 troy ounces of zinc to obtain 65·55 ounces of gold—over 50 times as much as should be necessary. There is something like a corresponding loss of cyanide of potassium. These wastes I have gathered from the data furnished by Eissler in his paper on the "Cyanide Process, &c.," read before the Institute of Mining and Metallurgy on November 21st, 1894. Free alkali, or an alkaline condition of the cyanide solution, is desirable during the solvent action on the auriferous material; but it is undesirable in the zinc precipitation, on account of its solvent action on the zinc, and its tendency to tarnish the zinc so that the auric cyanide may not readily act on it. On that account the smallest possible amount of alkali is used, when more would be desirable in the solving stage, for decomposing metallic salts, &c., in tailings.

When the depositing vats are cleaned up the precipitated gold is much mixed with pieces of zinc, insoluble zinc salts, &c. After careful drying, the zinc and gold slimes are treated by oxidising and smelting processes to secure the gold bullion, which is far from fine—say 700 out of a possible 1,000.

Dr. Keith.

With the object of overcoming the enumerated troubles, and other disadvantages, several experimenters have devised—on paper, at least—electrolytic processes for obtaining the gold from cyanide solutions. So far, the most successful one in extensive practice is that known as the "Siemens and Halske "process," which is described in a paper read by Von Gernet before the South African Metallurgical Society some months ago, in the before-mentioned paper by Eissler, and one by Butters and Smart, hereinafter noted. From these I gather the following facts:—

The cyanide solution is treated in four electrolytic vats, each measuring, approximately, 20 × 4 × 8 feet, containing, alternately arranged, anodes and cathodes, which measure each 4 × 8 feet, and are placed so that the solution in flowing through the vat moves downward on one side of each electrode and upwards on the other side, or are so arranged that the solution moves upwards by both sides of several electrodes and downwards by a like number, serially, thereafter.

The anodes are plates, or sheets, of iron, enclosed by canvas, and the cathodes are thin sheets of lead, supported in and by wooden frames. The current of electricity is caused by a dynamo taking 5 H.P. mechanically, and yielding 3½ H.P. in electrical energy at 4 volts pressure. (Von Gernet says 4, Butters and Smart say 10, while Eissler says 7.) According to these data, the electric current may be $\frac{746 \times 3\frac{1}{2}}{4} = 652{\cdot}75$ amperes.

The amount of iron used was 1,080 lbs. in one month, and the amount of gold deposited in that time was 697 oz. 15 dwt. 15 grains. The resulting products are the above amount of gold, some Prussian blue, ferric oxide, hydrogen, and an amount of impure litharge (Pb O) from the cupellation of the lead cathodes; but nothing except the gold is of any economic value, because of the expense of reducing the other products to re-workable conditions.

The facts about this process furnish an excellent text for a detailed consideration of the principles involved in the electrodeposition of gold. To begin at the beginning:

Dr. Keith. *The Vat, or Depositing Vessel.*—It is rightly made large, so that it may contain sufficiently extended surfaces of electrodes. It is made of wood, which sufficiently resists any deteriorating action of the electrolyte used in it. The electrodes are rightly arranged so that as the electrolyte passes through the vat, from end to end, it may come, as far as possible, into actual contact with the surfaces of the electrodes. Whether the surfaces are large enough depends upon the amount of electrolyte which passes through the vat in any unit of time, and whether, in its motion, it is brought actually, molecule by molecule, into contact with the electrodes. From the statement of the amount of gold—4 dwt. 8 grains per ton of solution—remaining in the electrolyte after passing through the depositing box, we may doubt the sufficiency of exposure to contact. The cathode surface is stated by Eissler to be 1,566 square feet. But, as it is evident from his other figures that he only took into account one side of the cathodes, the actual surface is 3,132 square feet. This is for one of four "precipitation," or depositing, vats. So we have 12,528 square feet of lead surface, upon which were deposited 697 ounces of gold in one month. This statement of cathode surface agrees closely with that stated by Von Gernet, and by Butters and Smart in a paper on the cyanide process, read by the latter two before the Institution of Civil Engineers, February 19th, 1895. The iron anode surfaces are $89 \times 7 \times 3 \times 2 \times 4 = 14{,}952$ square feet, from which we must deduct $7 \times 3 \times 2 \times 4 = 168$ square feet of surface of the end plates not presented to an opposite cathode. The anode and cathode surfaces are about $1\frac{1}{2}$ inches apart. On the whole, the *mechanical* construction of the vat, and the arrangement of its parts, seem to have been carefully considered, and to only err in the amount of electrode surfaces being too small for the *actual* electrolysis which takes place. The *effective*, or *commercial*, electrolysis is a very much less quantity, as we shall presently see. Von Gernet says that a better effect is produced by doubling the surface of electrodes than by increasing the current ten-fold.

The Electrolyte is, of course, the solution of gold in a liquid composed of water, carrying also small percentages of potassic

cyanide, potassic hydrate, and various compounds of cyanogen with metallic substances previously existing in the ore which had been treated by the solution, now the electrolyte. By the action of the electric current some ferro-cyanide of potassium is formed in the electrolyte, though much of this is converted into Prussian blue by contact with the oxide of iron formed on the iron anodes. The obvious improvement to be made is avoidance of any iron actions and reactions, because some of them consume electrical energy. The electrolyte is a weak solution at best, and, therefore, specifically, a poor conductor. But, as the quantity is at all times large, the cross section being 6,000 square feet, and length between electrodes but 1·5 inches, the resistance is practically *nil*, and need not be considered. The main consideration, as to resistance to electro-motive force, is that offered by the counter electro-motive force of polarisation. The oxide of iron formed on the anodes, and in the pores of the bags drawn over them, is, as a matter of resistance pure and simple, of some importance. Dr. Keith.

The Anode is composed of sheets of iron one-eighth of an inch thick, exposing, in the four vats, 14,784 square feet to electrolytic action. Each of the 356 iron plates is "cased in canvas to retain "the small quantity of Prussian blue produced" (Butters and Smart), but also to retain the *large* quantity of ferric oxide produced. Von Gernet and Eissler state that the object of the "canvas is to prevent short-circuits." This last is an obvious precaution and result, owing to the close proximity of anode and cathode. We gather from the papers cited that an iron anode has been adopted after trying both carbon and zinc. Eissler says: "Carbon could be used as an anode, but it will not with-"stand the action of the current, and soon crumbles into a fine "powder which decomposes cyanide. This finely divided carbon "is in suspension, and cannot be removed from the solution by "filtration."

Weightman states that he has "been using 'good arc-lamp "carbons,' and carbon in other forms, for the last six months, "and they are still in excellent condition."

Von Gernet says in his paper on the subject read last October before the South African Metallurgical Society, at

Dr. Keith. Johannesburg: "Zinc used as an anode forms a white precipitate "of ferro-cyanide of zinc by the reaction of zinc oxide upon ferro-"cyanide formed during the leaching. Similarly, iron anodes form "Prussian blue by the reaction of oxide of iron and ferro-cyanide."

Let us consider these anodes. Owing to the fact that there must be an analysis (decomposition) of the electrolyte at the anode, electric energy is absorbed, or rendered latent, there; and if the material of the anode does not combine with the anion set free, to form a compound therewith, that energy is lost. Generally, and in this case, oxygen is directly or indirectly the anion, and as it cannot combine with a carbon anode, for instance, it escapes from the anode to the air as gas. The carbon anode is practically *chemically* unacted upon by the oxygen anion, but is *mechanically* disintegrated thereby, and the more rapidly the greater the current, and therefore of the escape of gas. Carbon is objectionable as an anode pure and simple, because of these two properties. But it is further objectionable because the oxygen, reacting on the potassic cyanide, forms potassic cyanate, which is inoperative as a solvent of gold. So, not only is the energy lost, but also the active ingredient of the electrolyte, making it inoperative as a subsequent solvent. Von Gernet's statement as to zinc is not clear. A very little ferro-cyanide should be formed during the leaching process. More likely the "white precipitate" is an insoluble coating of zinc oxy-compounds with cyanogen formed on the zinc anode. This coating offers great resistance to the electro-motive force, so that it has to be increased to pass the necessary current; and this in its turn increases the difficulty. Much oxygen is set free, causing the corresponding wastes. So far, we now have only the iron anode to consider, as it has been adopted, and is used, at the mines. Practically, the iron anode is oxidised by reason of the current, and it takes a definite amount of electric current to oxidise a definite amount of iron; the iron-ampere-hour being 16·3 grains. The amount of iron used in the month of August was 1,080 lbs. This equals $1{,}080 \times 7{,}000 = 7{,}560{,}000$ grains. Providing this was all oxidised, the ampere-hours were $\frac{7{,}560{,}000}{16 \cdot 3} = 463{,}803$.

According to Von Gernet, the amperes were produced by 4 volts; Dr. Keith.
therefore there were in a month $\frac{463{,}803 \times 4}{746} = 2{,}487$ H.P.-hours.

The electric energy was $3\frac{1}{2}$ H.P.; therefore was used $\frac{2{,}487}{3 \cdot 5} = 710$ hours, in a month of 31 days, or 744 hours—a curious coincidence. But Eissler says the pressure was 7 volts; and Butters and Smart say the dynamo used gives 300 amperes at 10 volts. These latter statements, taken with the probable conditions that there are undoubtedly some wastes of iron in the way of unoxidised scrap, and possible intermissions in current, are probably nearer correct. Taking 300 amperes for 700 hours, we have an oxidation of iron $\frac{300 \times 700 \times 16 \cdot 3}{7{,}000} = 489$ lbs., and a scrap waste of $1{,}080 - 489 = 591$ lbs. Evidently we need some more exact information for theoretical considerations. For this current—assuming the minimum statement be correct—300 amperes, the gold deposit was 697 oz. 15 dwt. 15 grs. = 335,935 grains. According to our gold-ampere-hour equivalent of 38·1, this quantity requires 8,817 ampere-hours to deposit; but to do this $300 \times 700 = 210{,}000$ ampere-hours were expended—about 24 times too much, if the proper association of materials had been made so that all the economic actions went on in the electro-chemical equivalents to the gold. Upwards of 200,000 ampere-hours were expended in decomposing water and setting free hydrogen.

The Cathode is of sheet lead. It was adopted, according to Von Gernet, as best satisfying the following conditions:—

"1. The precipitated gold must adhere to it.
"2. It must be capable of being rolled out into very "thin sheets to avoid unnecessary expense.
"3. It must be easy to recover the gold from it.
"4. It must not be more electro-positive than the anode, in "order to prevent return currents being generated "when the depositing current is stopped The most "suitable metal for the purpose is lead, ... and "meets all the requirements of the case."

These four requirements are very good, and should be, with

Dr. Keith. others, carefully attended to. There is no exception to be taken to the first, within practical bounds. To the second, we say the cathode should be as thin as is consistent with mechanical stability; and most assuredly as thin as possible when wasted absolutely, as it is in practice, at the rate of 750 lbs. per month, in a small plant. A far better cathode is one which does not deteriorate, and practically is not wasted at all.

To the third, we say that it is not "easy" to recover the gold from the lead cathode. The necessary melting and cupellation is an expensive process—much more so than is the recovery of gold from the amalgam from a mercurial cathode, by distillation, or "retorting," as is continually practised in the metallurgy of gold. To the fourth, we say that a lead cathode *is more* electro-positive than an iron anode in a cyanide solution, and, therefore, does not fill the requirement. Furthermore, as soon as the lead cathode becomes coated with gold it becomes a gold cathode, which is electro-positive to the iron anode, therefore defective so far as this requirement is concerned. According to Von Gernet, a mercury cathode has been tried, but is impracticable on account of the large amount necessary to carry on the operation—say 80 tons to a small plant. "Sheets of solid metal (as copper) coated "with mercury have also been tried, but have been unsuccessful, "because the mercury, owing to the action of the current, will "penetrate the copper and form a dry amalgam, which does not "adhere to the plate." The mercury does not penetrate the copper plate any sooner than it does in the ordinary copper-plate amalgamation of the gold mills. That it forms a "dry" amalgam with the small amount of mercury which a vertically-placed copper plate will alone retain is evident. But the copper-plate cathode could be occasionally re-freshened with mercury by sprinkling to the extent of keeping the amalgam plastic, but not so thin as to run off. I prefer to furnish the mercury necessary on a copper-mercury anode by the direct electro-deposition of mercury with the gold from the electrolyte.

The Current.—This is variously stated by the different authors quoted as from 300 to 600 amperes with pressures of from 10 down to 4 volts. Without unnecessary wastes, the current

sufficient to deposit 697 oz. 15 dwt. 15 grs., equal to 335,935 grs., of gold in 700 hours is $\frac{335,935}{700 \times 38 \cdot 1} = 12 \cdot 6$ amperes. Von Gernet says: "In a fixed time a given electric current will "deposit a certain quantity of metal, which quantity varies for "different metals in direct proportion to their electro-chemical "equivalents. This law holds good only for solutions strong in "metal; but with dilute solutions, as in use in the cyanide process, "the current does not find sufficient of the metallic compound "present at the electrodes, and consequently decomposition of "water also takes place. For this reason, to make the efficiency of "the precipitation as great as possible, constant diffusion of the "solution is requisite." This explanation contains the essence of the whole matter. The efficiency is only from $\frac{12 \cdot 6}{600} = 2 \cdot 1$ per cent., to $\frac{12 \cdot 6}{300} = 4 \cdot 2$ per cent. In most of industrial operations we would consider such efficiencies exceedingly low. The waste of energy to produce the current, the waste of cyanide to produce Prussian blue, the waste of iron to make oxide and Prussian blue, the waste of lead to produce oxide of lead, and the waste of labour in handling these unnecessary quantities, add to the *necessary* expense of electrolytic treatment of auro-cyanide solutions at least £130 in a 3,000-ton per month plant, or 10·4d. per ton. Dr. Keith.

It will be asked, "Why these wastes, and can they be avoided "without substituting others equally as expensive?" These expenses are due primarily, and mainly, to the improper selection of electrodes. The anode should be soluble under the electric current in the electrolyte in which it is immersed, for at least two reasons—(1) that it may communicate to the circuit the energy due to the combination of the metal of the anode with the anion; (2) that the compound thus formed may be removed by the electrolyte to expose a fresh metallic surface for the continuation of the operation in regular progression.

Iron is not soluble in cyanide of potassium solutions. It is, as I have before stated, strongly electro-negative in such solutions to all other metals which may be met with in the operation.

Dr. Keith. The anode should be electro-positive to gold. Copper is so, but it would enter the solution at the expense of the cyanogen. Besides, it would be deposited on the cathode after a time and render the gold impure. Carbon is not suitable, because in the cyanide it causes the formation of cyanate, and, besides, it is strongly electro-negative to a gold cathode. Zinc is electro-positive to gold in the electrolyte; but, as before stated, there forms upon it an insoluble coating impenetrable to moderate voltage of electricity. With iron it is necessary to raise the pressure of the current to overcome the resistance of the oxide and Prussian blue on its surface. But if it be arranged so that the anode be soluble, the current may be of very low voltage; and even it may contribute energy to the circuit. The way to secure this *desideratum* is to provide an electrolyte for the anode which will promote its solution. To do this the selected electrolyte must be mechanically separated from the cyanide electrolyte. This can be done by the use of porous cells, such as are used in various of the forms of galvanic batteries. The electrolyte in the porous cell, for convenience called the anode-electrolyte, must be of such a composition that the accidental mixture of it with the cyanide solution, for convenience called the cathode-electrolyte, will not injure the latter, nor confer upon it deleterious qualities. Such an electrolyte is found in aqueous solutions of ammonium salts, such as the chloride, bromide, and sulphate of ammonium. Zinc or iron anodes placed in such electrolytes, in porous vessels immersed in the cathode-electrolyte, will be readily dissolved, by reason of the formation of soluble double salts of the metal with ammonium.

With an iron anode in the cyanide solution it is necessary to raise the voltage to cause the current to pass the oxides, &c. In doing so there is an evolution of much free oxygen, with corresponding loss of electrical energy. But that is comparatively a minor defect. It also sets free a corresponding amount of hydrogen at the cathode. These two gases accumulate, each on its appropriate electrode, until their comparative levity causes them to escape against the pressure of the electrolyte. But they continually cover a very large part of the electrode surfaces, and thus keep from them the electrolyte, which should be in contact

therewith. If this action be prevented, the electrode surfaces Dr. Keith. need not be nearly as large for the same amount of deposition of gold. In order to force the deposition, then, the voltage is raised to from 4 to 10 volts, though one-quarter of a volt is sufficient to produce the necessary actions and reactions under proper assemblages of materials as to quality, size, and position. If the analysis (decomposition) and synthesis (composition) be equal, no energy is absorbed other than to overcome the resistance. As the resistance is small, owing to the large cross section of electrolyte and small distance between electrodes, a very small electro-motive force is necessary when the proper electrode, and electrolyte for it, are used. Take such an assemblage, and the work of depositing 697 oz. of gold should be done with the expenditure of $12{\cdot}6 \times 0{\cdot}25 = 3{\cdot}15$ watts, for 700 hours, instead of $300 \times 10 = 3{,}000$ watts for the same time.

If iron had been used in a proper electrolyte, the iron dissolved would have been as 65·5 : 28 :: 697 : 298 oz. of 480 grs., equal to $\frac{298 \times 480}{7{,}000} = 20{\cdot}4$ lbs., in place of 1,080 lbs.—a saving of 1,060 lbs. But iron would have used, say, about 40 lbs., including waste, of ammonium sulphate in the anode-electrolyte—worth about 55 pence.

If zinc had been used in a suitable electrolyte, the amount would have been 65·5 : 32·5 :: 697 : 345 oz. of 480 grs., equal to 23·6 lbs., with a like consumption of ammonium salt. By the direct zinc precipitation in the McArthur-Forrest process, according to Eissler, there is a consumption of zinc varying from 0·25 to 0·50 lb. per ton of ore—say an average of 1,000 lbs. for the quantity of gold produced (697 oz.) from 3,000 tons of auriferous tailings. There is in this item alone a saving possible of £18 sterling per month.

In the *Proceedings of the Engineers' Club of Philadelphia*, for November, 1894, there is a paper, entitled "The Electro-"Metallurgy of Gold and Silver," by A. L. Eltonhead, in which he describes an alleged process of separating gold from cyanide solutions, said to be used in a "small plant" by "the electro-"chemical process (patent applied for) on the west side mine of

Dr. Keith. "the Tombstone Mill and Mining Company, of Tombstone, "Arizona." The writer of the paper was evidently not fully posted as to the process, and did not mention the name of the "inventor" (?).

The description is somewhat vague and imperfect; but we gather that the "precipitating box is of novel construction;" is 10 feet long, 4 feet wide, and 1 foot high, divided into "five "lengthwise compartments." "Under each partition grooves may "be cut, a quarter to a half inch deep, extending parallel with "the partitions, to serve as a reservoir for the amalgam, and give "a rolling motion to the solution as it passes along through the "four [*sic*] compartments. The centre compartment is used to "hold the lead, or other suitable anode, and electrolyte." "The "electrolyte may consist of saturated solutions of soluble alkaline "metals and earths. The sides or partitions of each compartment "dip into the mercury, which must cover the 'box' evenly on "the bottom to the depth of about a half inch. Amalgamated "copper strips or discs are placed in contact with the mercury, "and extend above it, to allow the gold and silver solution of "cyanide to come in contact. The electrodes are connected with "the electric dynamo, the anode of lead being positive and the "cathode of mercury being negative. The dynamo is started, "and a current of high amperage and low voltage is generated, "generally 100 to 125 amperes, and with sufficient pressure to "decompose the electrolyte between the anode and cathode. As "the gas is generated at the anode a commotion is created in the "liquid, which brings fresh and saturated solution of electrolyte "between the electrodes for electrolysis, and makes it continuous "in its action."

There seems to be a distinction between the "electrolyte" and the "solution of double cyanide of gold and potassium;" but where they are separated and how, if at all, does not appear. There is a "half-tone" picture of the "box," but it gives no more information than does the above description. The anode surface is about 16 to 18 square feet, which can be made less by raising with thumb-screws. The cathode surface may be about 40 square feet.

Dr. Keith.

With a current of 125 amperes we should judge that "the gas "creates a commotion," and such a commotion that it would be wonderful if the process be speedy, and otherwise than "continuous."

From the principles involved, which we have considered, this "process" may be set aside. Yet it is but one of the many suggestions, or attempts, made by "inventors" (?) unacquainted with the electrical and chemical requirements. We have not the time to consider more of these within the limits of this paper. We know that oftentimes more is learned through failures than through successes, so we may consider these and the fallacies at some other time.

After thorough experimenting and test I have devised, and patented throughout the gold-producing countries of the world, the following described process for electrolytically obtaining gold and silver from auriferous and argentiferous rocks and ores:—Sufficient solution of cyanide of potassium, of a strength varying from 2 to 10 lbs. of cyanide to each ton of water, with from 2 to 10 oz. of a soluble salt of mercury, is prepared. The finely pulverised ores, or tailings, are submitted to the leaching process with this solution, as in the other cyanide processes. The electro-positive gold in the tailings decomposes the mercury salt which comes in contact with it, and thereby produces voltaic couples of mercury and the minute particles of gold, thus hastening the solving of the gold.

As the speed of solving is dependent upon the actual contact of the gold and mercury with the cyanide of potassium, it is preferable to keep the solvent in constant motion during the percolation, or leaching, so that fresh solvent, not exhausted of its cyanide, may continually be in contact with the gold. To accomplish this, the whole, or part, of the solvent is frequently drawn from the leaching vats, or is continuously passed through the tailings, until the solving action is completed. In this way much weaker solutions may be used than is the case where the cyanide solution is allowed to stand for long stated periods in the tailings. The weak solutions are preferable because the losses in material are not so great, and they are equally as good solvents as the strong, though not so speedy.

Dr. Keith. As the solutions come from the leaching vats they are run continuously through electrolytic precipitating, or depositing, vats, constituted as follows:—Long boxes are provided, say a little over 2 feet deep and 2 feet wide, containing copper plates 24 inches square, and crossing the boxes every 1½ to 2 inches, but placed so that every alternate plate rests on the bottom and sides of the boxes, and the intermediate plates are raised about half an inch above the bottom of the box.

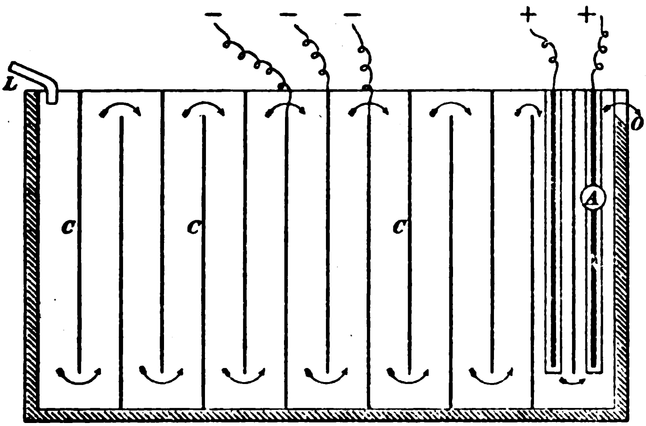
The number of plates and the length of the boxes are sufficient to permit of the deposition on them of all the metal constituents of the solution without so great a density of current that hydrogen is deposited likewise. If the solution be kept sufficiently in motion, the density may be 0·1 ampere per square foot of cathode surface; but, as the solution becomes much weaker in metal towards the latter end of the vat or box, the density there cannot be so great. But this matter is self-regulating, because the greater tendency to polarisation impedes the passage of the current at the latter, and weaker, end.

On the average basis of a density of current of 0·06 ampere per square foot, a cathode surface of 800 square feet is ample for a plant treating 3,000 tons of average tailings per month. One hundred plates of the size named supply this amount of surface. These plates are preliminarily amalgamated with mercury, and are each connected with a 'bus-bar which is connected with the negative pole of the source of electricity.

Between these cathodes are placed porous cells filled with a half-saturated, or other strength, aqueous solution of an ammonium salt, such as the chloride or sulphate. Into each cell is placed a rod, strip, or prism of zinc or iron, and each of these is connected by wire to another 'bus-bar, which is connected with the positive pole of the source of electricity.

The accompanying diagram shows a longitudinal section of a depositing vat. The auriferous solution enters the vat through the pipe L; it passes under the first copper-plate cathode C, over the second, under the third, and so on through the vat in the direction shown by the arrows, until it flows out at O. In the spaces between the cathodes C are the porous cells containing

rods or other forms of zinc, as at A. In this diagram the parts are not all shown, and the proportions are not the actual ones. The practical vat is much longer. The + and – signs designate the appropriate connections from the source of electricity. Dr. Keith.



The cyanide solution of gold and mercury is passed continuously through this vat, tank, or box, in a steady stream, and at a rate which ensures the deposition of a greater part of its metallic contents. If the quantity of mercury on the copper plates, together with that which is deposited from the solution, be not enough to keep the amalgam in a sufficiently plastic condition, more mercury solution may be added to supply mercury for that purpose. If it be too soft the amount of mercury solution may be decreased. No mercury is lost, because recovered by the electrolytic deposition.

Because the gold is deposited in exceedingly attenuated particles, the amount of mercury necessary to form with it a plastic amalgam is much in excess of the quantity of gold—say three or four times as much. This relation varies with the rate of deposition of gold. Take, for example, 12 6 amperes to deposit 1 oz. of gold per hour, and 29 amperes to deposit 3½ oz. of mercury in the same time, and we have the practical requirement of 41·6 amperes to deposit 1 oz. of gold per hour. This for 744 hours per month is enough to more than deposit the 697 oz.

Dr Keith. produced per month by the use of 300 amperes for the same time, in the case illustrated by the authors referred to hereinbefore.

Because the anode is rendered soluble by putting it in a suitable electrolyte there is no consumption of energy in the actions which there take place. There is no escape of uncombined or free oxygen. A zinc anode is electro-positive to the mercury cathode to the extent of 0·25 volt—in itself sufficient to cause the deposit. When the anode-electrolyte has become exhausted as an electrolyte by the formation of double salts with the metal of the anode, the porous pots have their liquid contents removed and new electrolyte substituted therefor. Porous pots are used an inch or so in diameter and 24 inches deep; eight or ten in each space between the cathodes are sufficient.

The cathode, though in its substance mainly of copper, is, electrically, a mercury one; therefore, not as strongly electro-positive in its electrolyte as is a copper one. But the anode is sufficiently electro-positive to it to keep the cyanide solution from dissolving the gold and mercury when the dynamo, or other source of electricity, is unconnected, provided the anode and cathode be connected electrically.

The depositing vat can be, and is preferably, worked continuously. The anode parts are supplied from time to time as fast as dissolved. The anode-electrolyte is renewed from time to time, as necessary, by the use of a hand pump. The amalgam is removed whenever desired from the cathode plates. For this purpose they can be disconnected from the 'bus-bar one by one, removed from the vat readily, the amalgam scraped off, and the plates returned to the vat. There is, necessarily, no wholesale removal of electrodes, as, for instance, in the previous case, when the iron-plate anodes become full of ragged holes from corrosion, or when the lead cathodes were removed to be melted and cupelled. The amalgam is "retorted," as in the usual routine of gold amalgamation—a much simpler and less expensive operation than melting and cupelling. Practically no mercury is lost; but lead cathodes are lost.

The electro-motive force necessary to carry out this process does not exceed one-half volt. Whatever there be more than

that would be expended in decomposing the water of the electrolyte, thus unduly oxidising the anode, and setting free hydrogen at the cathode to no good effect; but, rather, cover the cathode with hydrogen films, and to that extent reduce the surface available for receiving the metal deposit, thus increasing the resistance and lessening the current. In the effort to increase the metal deposit by raising the voltage we would meet the same difficulty as is experienced with the said Siemens and Halske process, and find, as Von Gernet says, "a better effect "is gained by doubling the surface than is obtained by increasing "the current ten-fold." Dr. Keith.

The expense of working 3,100 tons of tailings in August, 1894, at the Worcester Mill, in Johannesburg, by the Siemens and Halske process, as stated by Eissler, was, in detail, as follows:—

	£		d.	
Filling and discharging leaching vats	125	monthly	10	per ton.
Cyanide	75	"	6	"
Lime	15	"	1·2	"
Caustic soda	6	"	0·5	"
Lead	14	"	1·1	"
Iron	28	"	2·2	"
White labour...	65	"	5·2	"
Natives' wages and food	20	"	1·9	"
Coal	57	"	4·6	"
Stores and general charges	41	"	3·2	"
Total	£450	or	3s.	per ton.

By the improved electric process at least £130, or 10·6d. per ton, can be saved in expense in the following items:—One-half of the cyanide, all of the lead and iron, 10 per cent. of the labour, and three-quarters of the coal.

There would be wasted by the new process, say, 25 lbs. of zinc, worth at the mines about 19 shillings, and about five to ten shillings' worth of ammonia-salt—about 0·11d. per ton.

The use of strongly alkaline solvents is desirable during the dissolving stages of a cyanide process, because the alkali serves

Dr. Keith. to decompose various salts and oxides, such as ferrous sulphate and oxide, ferric sulphate, and sulphates of aluminium and magnesium, &c., which would otherwise decompose the cyanide.

But alkali is inadmissible when zinc precipitation follows, because of the increased waste of zinc caused thereby in the formation of zincate of the alkali.

The alkali is inadmissible when chlorine, iodine, bromine, and such like "cyanicides" are used, because they expend their action on the alkali instead of where desired.

By my electrolytic process the solvent may be very strongly alkaline without detracting from its desirable solvent action, but, rather, adding to its efficiency, and decreasing what would otherwise be the solvent action on other constituents of the ores.

The President. The PRESIDENT: Gentlemen,—I am sure you will join with me when I propose a vote of thanks to Mr. Keith for his very interesting paper. A good many gentlemen I know can give us some very interesting details upon this matter. The time is very short, and I will therefore not say anything more, but will call upon you to give your hearty vote of thanks to Mr. Keith.

The vote of thanks was unanimously accorded.

Mr. Vautin. Mr. CLAUDE VAUTIN: I have listened with the greatest possible interest to the paper of Dr. Keith on such an interesting and fascinating subject as the electrolysis of gold. I am afraid I shall have to cross swords with Dr. Keith. I should like to make a remark first to this effect: I think that the Doctor has been remarkably unkind to all other inventors, for several times throughout the paper he refers to them as having no knowledge about chemical or electric matters, and so on. Therefore I must at once consider that the Doctor himself poses as a very high authority on the subject. Now the paper, other than being on Dr. Keith's process, is a description of several other processes which are well known to most of us here, and we have had a lot of detail as to very well known and well-established chemical and electrical facts—probably interesting and refreshing now and again. As the time is short, I will refer as quickly as possible to a statement in the paper that I question very much. Dr. Keith states: "I have discovered that mercury is electro-negative to gold in a

"solution of cyanide of potassium." If the Doctor has built up his hopes and process upon that discovery, I am rather sorry to say what I have to say now, and that is this—that Dr. Gore published in the *Proceedings of the Royal Society*, 1879, the result of some investigations on the subject, and pointed out that mercury was *negative* to gold in solutions of potassium cyanide. In 1876 a most excellent paper by Skey was published in New Zealand referring to the application of cyanide of potassium as a means of assisting the amalgamation of gold when associated with iron pyrites, and it was pointed out that gold was so electro-positive to mercury that it was dangerous even to allow mercury with gold and silver in it in the form of amalgam to remain in contact with a solution of cyanide of potassium, because the gold and silver *would dissolve and the mercury would not.* Mr. Aarons, in an admirable paper on gold milling published in 1889 in the *Engineering and Mining Journal* of New York, states: "I would suggest as a means of improving the percentage of gold "recovered as an amalgam when treating ores and concentrates "in pans, that cyanide of mercury dissolved in cyanide of "potassium should be used. The best way [he states] to make it "is to dissolve red oxide of mercury in cyanide of potassium. "As, however, this solution dissolves gold very easily and very "rapidly, it is only right that precautions should be taken to "precipitate the gold dissolved by the mixed cyanide of "potassium and mercury during the operation, and probably the "zinc amalgam described and used in the pan will do that." Further, the very process described by the Doctor—namely, dissolving the gold from ores by a solution of cyanide and mercury dissolved in cyanide of potassium—has been used more or less for the last two years in Hungary for the same purpose; but in practice it was found that whenever the gold grain had any perceptible size, the very fact of which the Doctor has made so much (the property of gold to precipitate mercury on its surface) actually retarded any further action of the cyanide of potassium on the gold. Experiments were then made with the same ore with potassium cyanide without the mercuric cyanide, and the results were slightly in favour of cyanide of potassium cyanide Mr. Vautin.

Mr. Vautin. alone. I am quite sure the Doctor has missed the literature on the subject, and the fact I have referred to, otherwise he would not, I am sure, have claimed the discovery that gold was positive to mercury in the solution of cyanide of potassium—that is to say, in a solution of double cyanide of potassium and mercury dissolved in a solution of cyanide of potassium.

There are many other points I should like to take exception to, but I am afraid it would take up too much time, and I will confine myself to a few important ones. The paper is so absolutely devoid of any data as to the results the Doctor has obtained, that we are prevented from comparing existing processes with the Doctor's. I should like to know what the result has been of passing, say, 10 tons of his solution containing 2 dwts. of gold to the ton of solution obtained by leaching a suitable ore with cyanide through the apparatus as described, or, in other words, what amount of gold was contained in the solution when it entered the Keith decomposing vat, and what amount in solution when it passed out of the vat. What was the percentage of the *available cyanide* of potassium when the solution entered the vat and when it left the vat? I should like to know if the Doctor ever precipitated anything like three ounces of gold on an amalgam copper plate of the size described by electrolytic process, and, after having precipitated it, if he has attempted to remove it in the form of amalgam by the ordinary way of rubbing and scraping the plates. Then, again, the Doctor has made one statement with reference to his particular process that is open to doubt. He states that the plates when used as proposed by other inventors become dry, and that as he throws mercury down simultaneously with the gold they are kept wet. Surely each time the solution passes through the depositing vat the mercury contained therein would be deposited with the gold, considering the current used, and be removed from the solution which he proposes to pass through the ore under treatment a second or third time; for special mention is made of the advantage of circulating the solution through and through the ore. If this is the case, it would be necessary to add a fresh quantity of cyanide of mercury

each time the solution was pumped on to the ore, otherwise there would be no mercury remaining to assist the dissolving of the gold after the first wash had passed through the ore, and thus a far larger amount of mercury salt would be required than described. I am pointing out these one or two facts from a practical point of view. I say, speaking generally, if I can obtain better results than now obtained in as simple a way, then I will change my methods; or I must be shown that I can obtain the same results at a cheaper cost before I alter the present method. I take it neither Siemens and Halske nor the Doctor have established the fact that any electrolytic process that has been suggested as a means of practically recovering the gold from cyanide solutions has been proved to be one iota better than the method that has been, and is, in general practice, viz., of simply using zinc. Much has been said to the effect that much weaker solutions of cyanide can be used when electrolytical precipitation is used; but the second washings in the ordinary process are much weaker in gold and in cyanide than any of the first washings obtained in the electrolytical methods. The suggestion is that a much weaker solution can be used for dissolving gold, but you cannot precipitate your gold from a weak solution by zinc, but you can by electrolysis. I beg most respectfully to differ from that opinion. I have actually seen gold solutions containing only 1 dwt. to the ton passed through zinc, and when properly handled less than ½ dwt. per ton was in the liquid when it passed through the zinc boxes. I do not think any electrolytic process can do more, or as much, at the same cost. What about the 24-in. porous cells between each cathode? I should like the Doctor to tell us where he gets them from, and how long he thinks they are going to stand up under the conditions he has named. What happens by the diffusion of the sulphate of ammonium through the cyanide of potassium? Is not cyanide of ammonium formed? I want to know the analysis of the solution, or, rather, the percentage of free and available cyanide when it enters the apparatus and when it passes out. It is a most important matter. I want to know if he can give us any data to substantiate the statement here that the "*more* Mr. Vautin.

Mr. Vautin "*alkaline the solutions are when you are dealing with your ores* "*the better.*" It is an established fact in practical working that if you have any excess of free alkaline at all you run a great risk of forming alkaline sulphides, which are hurtful in many ways. Of course you never find any ore just as the inventor of a process would like. These alkaline sulphides, if there is a certain percentage in the cyanide of potassium, immediately prevent any further action of the cyanide on gold and silver. If you have alkaline sulphide in the presence of cyanide of mercury, what happens? The mercury will be left behind in the ore in the form of sulphide. I am speaking thus of the paper with the very best possible feeling; but if I may be allowed to say so—and I do so with all respect—the whole paper seems to be one of finding fault with other people, and without giving us one iota of information by which we can favourably compare the process brought before us with others that have gone before. I should be delighted to get away from the zinc precipitation in the cyanide process, but I want more information on the subject than has been laid before us to-night before being off with the old love. I wish the Doctor had given us some results of his own work on the subject. I should like to know one thing more. Has the Doctor ever taken a 0·01 solution of potassium cyanide, with the requisite quantity of cyanide of mercury, and passed that solution through ore, and then through his decomposing box, then brought it up to normal strength and repassed it through another lot of ore? and what has been the result of such operation as to gold extracted?

Dr. Rideal. Dr. RIDEAL: I have really very little to say, as I came more to listen to what would follow in the discussion than to take part in it myself; but I have made one or two notes on the paper. In the first place, some of the phrases used by the author in the earlier part of the paper are rather curious. I notice in the second paragraph, for instance, the phrase "about the size of an "atom," which is a most unscientific phrase for a chemist to find in an electro-chemical paper. Later on, there is the use of the term "coulomb of current." Surely "coulomb of quantity" would be a better phrase than "coulomb of current" here; and

the term "hydrogen-coulomb" is quite a novelty, which I have Dr. Rideal. not met with before. The author also refers to the text-books. I think that in most text-books one finds that these series are only true in given liquids; or, if no liquid is mentioned, the series refers to the difference of potential of the metals in air. I think, therefore, that his criticism requires amendment. With regard to the process itself, it seems to me the essential novelty of the author's process consists in the introduction of this mercuric cyanide into the solution, and Mr. Vautin's criticism on that, of course, is important in that connection; but still, looking at it from a theoretical point of view, one would think that the addition of mercuric chloride to a solution would facilitate the solvent action of the cyanide of potassium on the gold. With regard to the recovery of the gold from the solution in the process described, it would seem that, theoretically at any rate, no extraneous source of energy would be required; for one could conceive a cell, such as described, in which the zinc plate in the porous pot would be connected up with the amalgamated copper, and this couple would in itself be sufficient to ensure the deposition of the gold upon the mercurial copper. With regard to the removal of the zinc from the gold solution which the porous pot effects, Becquerel describes in his classical book on "Electro-Chemistry" (in which one sees several of these modern ideas in perhaps a crude form, but still there in the germ) a very similar experiment for detecting small quantities of gold, and also goes on to say that the process is one which can be used for recovery of gold from solution. I think his experiments must have been before the time of porous pots—some 50 years ago—for, instead of using a porous pot, he uses a funnel, plugged with some wet clay, tied on with a piece of linen, for his porous diaphragm. The gold solution is on one side of this diaphragm, and within the porous pot is a solution of sodium chloride, within which the zinc plate is placed. Then he uses for the deposition of the gold either a wire or a plate of platinum, or mercurised copper. So that in that experiment we have practically the same conditions as are obtained in the process described by the author. The author also mentions

Dr. Rideal. that he had tried carbon in the place of zinc in the earlier working out of his process, and proved that carbon in contact with gold and cyanide solutions facilitated the solvent action of the cyanide upon the gold. Although the carbon is electro-negative to gold, and therefore may act voltaically, it seems to me that when a small quantity of porous carbon is introduced—he mentions particularly in the paper that porous carbon is better than large—the oxygen absorbed in the carbon itself would aid the absorbent power of the cyanide solution, and that that action must contribute to the good results obtained by the use of carbon. It is a very ingenious process, but, of course, the proof of the pudding is in the eating. As Mr. Vautin has just said, we should like to have had some of the actual results obtained in working the process on a commercial scale put before us.

Mr. Swinburne. Mr. J. Swinburne: A great deal of this paper does not concern us directly as electrical engineers; the portion that is important from an electrician's point of view is the deposition of gold from the dilute double cyanide solution by electrical means. We have to compare two methods. In one, zinc shavings are used to deposit the gold by simple substitution. This seems simple, but we are told the solution of zinc, and, with it, the consumption of cyanide, is excessive. I would suggest that this extra consumption is due to access of air, or dissolved oxygen, and might be prevented. I do not know enough about practical gold-mining to know if it could easily be prevented. On the other hand, electrolysis must at least use up the equivalent of zinc, if a zinc anode is employed. If a carbon anode is used, it is acted on chemically (not mechanically) to some extent, and, in addition, cyanate of potassium is formed. In trusting to electrolysis a larger current than that corresponding to the rate of deposition of gold must be employed to secure complete deposition, and this means extra consumption of cyanide, and of zinc if that metal is employed as anode. Though naturally anxious to see electrolysis employed wherever possible, I cannot in this case see its superiority to the simpler method of deposition by zinc shavings.

The almost comical happy-go-lucky methods which appear to

be used at mines are not likely to give the best results. It would surely cost less to do things in an accurate and reasonable way, and to make tests and electrical measurements so that the results can be known and compared. Mr. Swinburne.

Mr. DESMOND FITZ-GERALD: I should like to make a few observations from the point of view of the student and of the inventor. One of several reasons why we should be thankful to Dr. Keith for his paper this evening is that he has condemned in no uncertain terms an hypothesis which has been prevalent amongst both students and inventors—viz., that cyanate, and not cyanide, of potassium is the active agent in the solution of gold. It has been correctly stated this evening that K Cy O is quite ineffective in dissolving gold. In regard to the voltaic relations of metals in solution of cyanide of potassium, these are so different from what obtains in most other solutions that many of the younger members here would be interested to hear something as to these relations, and also as to the usual potential differences obtained. Dr. Gore has given the electro-chemical order of metals in very weak cyanide solutions—solutions containing 0·139 per cent. of cyanide. The order, beginning at the positive end, is magnesium, zinc, aluminium, copper, tin, lead, gold, and then silver. The exact position of silver in the list is a little questionable. Next come antimony, nickel, mercury, iron, platinum, and copper. Mr. Fitz-Gerald.

Mr. PICARD: I should like just to say a word or two in defence of Rae. I am sorry to see one of the earliest pioneers of this important industry run down in this way on the score of ignorance, because he did not possess so much knowledge as we possess nowadays. I will not say anything further about the first reason quoted as causing the failure of the process; but with regard to the second—"from the impracticability of causing the particles of gold "to remain long enough in contact with the anode to be acted on "electrolytically"—I say I do not consider it absolutely essential that a particle of gold in the ore should at any time come in contact with the anode, and I consider that electricity would be of value though that particle of gold never touches the anode. It has been conclusively proved by experiment that if a particle of gold be placed between two poles in a cyanide solution, and out Mr. Picard.

Mr. Picard. of contact with either pole, then that side of it which is nearest to the positive pole would be negative, the other side would be positive, and so on with any number of particles. The positive side is dissolved naturally, and there is a tendency to deposit gold on the negative side; but in these weak solutions gold is not deposited on the negative side so rapidly as it is dissolved from the positive side. So that electricity in that way is of value in increasing the solvent action even if the particle of gold does not come in contact with the anode. Not that I consider it of the least practical advantage, but I only mention the matter as of scientific interest. I will just refer to the chloride of gold. I do not think anybody would look at an electrolytic method for decomposing that, because there are such excellent precipitants in sulphate of iron, sulphuretted hydrogen, &c., but, best of all, sub-sulphide of copper. If possible, one wants to avoid having a dynamo on a mine, for the men are not very good electricians. I should like a little confirmation as to the value of cyanate of potassium as a solvent for gold. I believe there are members present who will agree with me in not being satisfied that cyanate is not a solvent of gold. I believe it is, though not to so great an extent as cyanide of potassium.

Mr. SWINBURNE: I have tried it, and found it was not.

Mr. PICARD: I only asked for information, and accept the fact on your authority. I can confirm Mr. Vautin's remarks about the effect cyanide of mercury has upon the larger particles of gold in the ore. One more point: I think the term "cyanicide" is used wrongly. If Dr. Keith had been fully acquainted with the recent researches of Mr. Sulman on the action of bromine, he would not have called bromine a cyanicide. Certainly it should not be put in the same class as peroxide of sodium. I am very pleased to see an Institution like this taking up so important a branch as electro-metallurgy, because chemists have generally the impression that electrical engineers do nothing but electric light and motor work; and I feel confident that if electricians would combine with metallurgists more than is done at the present time we might achieve much more interesting results than can ever be obtained while working independently.

Dr. Du Riche Preller [*communicated*]: I am sure all electrical engineers who are interested in electrolysis of gold will congratulate the author on his very comprehensive paper, dealing, as it does, with all the latest and best publications on the subject, such as those of Von Gernet, Eissler, and McLaurin. Of course, electrical engineers, strictly speaking, are more especially interested in the electrical plant and apparatus; and it is very desirable that Dr. Keith should give some more detailed illustration of his apparatus than the mere sketch he has put on the blackboard, the more so as his process claims to be an improvement upon that of Messrs. Siemens and Halske. Dr. Preller.

But, even without such illustrations, he deserves great credit for having brought the subject before this Institution, the more so as a few weeks ago a discussion on the cyanide process took place before the Institution of Civil Engineers, when several speakers disparaged the precipitation of gold by electrolysis, on the ground that the process (Siemens and Halske's) was too delicate and complicated as compared with the chemical precipitation by zinc shavings. During the discussion on Dr. Keith's paper several speakers made remarks very much to the same effect.

Now I venture to point out that the electrolytical process is at once simple, economical, and efficient. The apparatus for precipitation, composed of a frame containing a series of thin metallic plates to which the electrodes are fixed, is just as simple as an ordinary storage battery. Again, the process requires only very dilute solutions of cyanide of potassium; whereas in the chemical, or zinc, process solutions 20 times stronger are used. And, lastly, by the very reason of dilute solutions being used, it is also more efficient than the zinc process, because, as has been pointed out also by Mr. McLaurin in his most recent paper (*Journal Chemical Society*, February, 1895, p. 211), the rate of dissolution of gold in potassium cyanide solutions is small for concentrated solutions, and increases as the solution becomes more dilute. It is true that with the electrolytical process a complete cycle of operations takes about 110 hours, or about 24 hours more than with the zinc process; but then the deposition

Dr. Preller. is much more effectual, and the electrical energy required (about 5 effective H.P. per 100 tons of solution) is so small that the cost is insignificant, the more so as the power is also utilised for other purposes.

I remember, when I had the privilege of working at electrolysis as a student in Professor Bunsen's laboratory, that illustrious chemist frequently emphasised the fact that electrolysis in all operations marks a great advance on chemical action pure and simple. Like everything that is novel, the electrolytical process for the precipitation of gold has to fight its way against both legitimate criticism and mere prejudice; but Siemens and Halske's process, as used at the Worcester Works of the Rand Ore Mining Company, in South Africa, has already emerged from its experimental stage, and, if such dividends as 25 per cent. are any criterion, has also proved a commercial success. Dr. Keith claims for his mercury process a saving of at least 25 per cent. over that of Siemens and Halske, and it is important that he should state in his reply whether, and where, his process has already been put to a practical test. If the saving he claims is an accomplished fact, and not merely a hypothesis embodied in a patent, he will have rendered a signal service to electrolytical science.

The President. The PRESIDENT: Gentlemen,—I am very loth to close this most interesting discussion, but time presses. I agree with Mr. Picard that we are to be congratulated on having this subject brought before us in a manner which shows that we are really interested in this most important subject. I think that it is much to be regretted that we have not had more frequent papers upon it. I have had some experience in the deposition of metals, but I only ask one question, which is, that when porous materials are used as proposed by Dr. Keith, does he not find difficulty in keeping these porous diaphragms, or pots, in order? I know that a great many substances have been tried, but anything in the nature of a porous pot has too high a resistance; and when you use vegetable or leather diaphragms they are very liable to be partially stopped up by deposits on their surfaces, so that they require attention and, hence, increased labour. This defect

was experienced in a very important copper-depositing process with which I have had connection. The method of turning the depositing tank into a voltaic cell, and thus utilising part of the energy, is very enticing, but it has the above-named practical disadvantage; and I must join with other speakers in saying that the paper would have a greatly enhanced value for us here if it had been a little more quantitative in character, and if we had known what would be the savings both in materials and labour set forth in more detail than has been the case. At the same time, I do not want to be at all grudging in thanks to the author for bringing this extremely interesting matter before us. I will now call on Dr. Keith to reply. The President.

Dr. N. S. KEITH: I shall have to ask for further time to make detailed replies to the various rather loosely presented statements made by Mr. Vautin, without specific dates or references. Like myself, the speakers to-night have dealt somewhat in generalities. I will say that this has evidently been long enough for one paper. I expect to acquire more knowledge in the matter, and hope some day to offer it to the electrical engineers for their consideration. I expected some unfavourable criticism, and, as usual, some laudation; therefore I am not at all cast down, and I am not elated. I must say that I did not enter into this matter in a captious spirit. I did not find fault with the old without the idea of offering something that was better. We never can offer anything better without finding fault with the old, directly or indirectly. I used the description of the electrolytic process of Siemens and Halske, which has been carried on for a considerable length of time in South Africa—and undoubtedly with success—as a text for illustrating the subject. It is not my fault that I found fault with it. It is the fault of the thing itself. I do not think the facts I have presented have been at all gainsaid. Whether there was 2, or 4, or 40 per cent. of efficiency, I do not present the fact in a fault-finding way, as fault-finding is generally understood, but simply to point out the only other electrolytic process which up to the present seems to have true commercial value. If that process can be improved, I firmly believe I have improved it, in spite of the little faults which some Dr. Keith.

Dr. Keith. of the speakers have found, which alleged faults I desire to consider either in a further discussion or in a written communication, as may be desired. Then questions will, perhaps, be more fully answered. Where I cannot answer them I will freely say so. I wish to say this—that this thing is not old enough to have had passed a great many tons through the process; but I have carried out work enough, without now entering into the details, sufficient to be satisfied that the thing is a practical, feasible operation. I shall be very pleased if Mr. Vautin, when he comes to revise the written report of his remarks this evening, will make them a little more definite and precise, so that I may by the next meeting, or some other time, answer them, as far as I am able, in full. Fault was found with my comparing the size of small particles of gold to something like that of atoms. Maybe it is one of those loose remarks which may be made for the sake of bringing the matter forcibly to the mind. These particles of gold are so attenuated that we do not know how small they are. In all probability, according to my own idea, they are all formed *in situ*, in the earth, by electro-deposition. Electro-deposition takes place in atomic quantities; therefore the small atom of gold may be deposited electrolytically in place, and increased thereafter by accretion of other atoms to form crystals. Therefore we cannot say how small these particles are. We know we find some exceedingly large, but we do not attempt to get these large particles by this process. The reason for using a separate source of electricity is to hasten the process. It can be hastened by the addition of a quarter to half a volt, and the requisite amperes, to the extent of density just below that which would cause the separation of hydrogen from the solution. In answer to Mr. Swinburne, I will say that zinc and gold couples decompose the water of the solution, and set free oxygen. I quite agree with him as to the loose methods which are practised in metallurgical operations at the mines, especially in the matter of gold. Certainly the McArthur-Forrest process, using cyanide of potassium with zinc precipitation, has brought more forcibly to the gold metallurgist the necessity of exact treatment, with close determinations of his quantities

and actions, than any other method of gold metallurgy. It has, from time immemorial, been exceedingly crude, and has only of late years become somewhat refined, and therefore cheapened. It has been done by the aid of the engineer, not the prospector, who simply understands a pick, a shovel, and a hammer. I did not state that oxygen does not combine with carbon; yet I know it has been a question, and it is possibly decided that it does. It will be noticed in that connection that I used the term "practically"—that practically the operation is the setting free of oxygen, which escapes into the atmosphere, and causes a mechanical disintegration of the carbon. As to the expense of using zinc: They use about 50 times more zinc in the zinc precipitation than is necessary in a true electrical or theoretical way. This excess is worth saving, especially at some mines where zinc costs so much. Dr. Keith.

Mr. Vautin seems to try to impress upon you that I set myself up as an exceedingly good authority in this matter, and that in my opinion everybody else who has preceded me was an absolute ignoramus. I did not mean to assume that position, and I do not think I did. The gentlemen who have preceded me in these matters were many of them learned men. I certainly cannot question the abilities of Messrs. Siemens and Halske, who are so well known, either in person or by reputation, to all of us; but that does not prevent their doing some imperfect and some bad work. We have all done it. In answer to Mr. Crompton's question about keeping the pots in order: The solution is an alkaline one; the action tending to make the pots imperfect conductors by filling their pores with insoluble deposits, which takes place in many electrolytic operations, and in batteries, does not prevail in this case. The cyanogen which is set free at the outer surfaces of the pots comes in contact with the alkali, and is immediately re-combined. The ammonium passes into the outer electrolyte, and, if the current be pushed to sufficient density, is set free as ammonia and hydrogen at the cathode. The cyanogen does not combine with the zinc, or the zinc salts, to form insoluble salts within the pores of the porous cells.

Dr. N. S. Keith [*communicated April 2nd*]: After having

Dr. Keith. seen Mr. Vautin's remarks in print, as revised by himself, I must say that I am but little better informed by him than before. His references are no more substantiated. Dates of alleged publications and uses are so imperfectly given that I cannot refer to the originals to see how far they bear out his allegations and deductions. For a long time, in 1866 and 1867, I used cyanide of mercury dissolved in cyanide of potassium to assist amalgamation of gold in amalgamating pans at the Eagle Mill of the Black Hawk Gold-Mining Company, in Gilpin County, Colorado; and I still believe it to be original with me at that time. I prepared my cyanide by dissolving mercury in nitric acid, evaporating to dryness, heating the nitrate to expel the acid, and dissolving the resultant oxide of mercury in a solution of cyanide of potassium.

Mr. Vautin is rather unfortunate in his statements, for they are contradictory. Taking, for the sake of the argument, that his references are correct as to persons, places, and dates, we are to believe that in Hungary the mercury deposited on the gold "actually retarded any further action of the cyanide of potassium "on the gold," during the last two years; while in New Zealand in 1876 "the gold and silver would dissolve and the mercury "would not," from an amalgam having undoubtedly much more mercury covering the gold than in the Hungarian case. Then Mr. Aarons, in 1889, says: "As, however, this solution "dissolves gold *very easily and very rapidly*, it is only right that "precautions should be taken to precipitate the gold *dissolved by* "*the mixed cyanide of potassium and mercury*." The italics are mine. The essence of these is to this effect—that I am not the discoverer, and that it won't work anyhow.

Mr. Vautin wants to know about a great many other items of interest, which I shall endeavour to put into another paper.

In answer to Mr. Picard, I have to say that I had personal acquaintance with Mr. Rae from 1865, and with his process and apparatus in 1888. To the electrician I need say nothing more in criticism than I did about the dynamos he used. It would be fully as ridiculous to charge storage batteries by the use of a series-wound dynamo as it was in his case.

Though perhaps not "fully acquainted" with the researches of Mr. Sulman on the action of bromine, I know enough of it to reiterate my belief about it as stated. Dr. Keith.

The word ELECTROLYSIS *is used in the full meanings of the Greek words from which it is derived*—ELECTRON, *from which came* ELECTRICITY, *and* LUSIS, *a release or setting free, or* LUCIN, *to dissolve, to loose, to set free. Therefore,* ELECTROLYSIS, *is the dissolving, loosing, or setting free, alternatively or collectively, of something by means of electricity. We electrolyse gold when we treat it by electricity and solvents to dissolve, loose, or set it free. It is true that the broadly-derived term "electrolysis" has become, by a succession of repetitions, confined in its applied meaning to decomposition by electricity, without consideration of the accompanying actions, which are as much electrolytical, as in electrotyping and electro-plating.*

N. S. K.

The PRESIDENT: I have to announce that the following candidates, whose names you have heard read, have been elected:—

Associates:

Henry Pem Bonus.	Robert Edward Phillips.
Herbert Edward Keen.	Mansingrao Apasahib Solasker.
Joseph Ernest Petavel.	William John Thomas.

Student:

Thomas Robert Robertson.

The meeting then adjourned.

The Two Hundred and Seventy-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, March 28th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on March 14th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Edward John Erskine.	Charles Pratt Sparks.

From the class of Students to that of Associates—

S. Allingham.	Thomas H. Minshall.
Edward Arnold Medley.	M. J. P. O'Gorman.
James Henry Millen.	William H. Wraith.

A donation to the Library was announced as having been received since the last meeting from Mr. Alexander Siemens, on behalf of the late Dr. Werner von Siemens. It consisted of the second volume of his translation of the works of Dr. Werner von Siemens, who had expressed a wish that when the work was published a copy should be presented to the Institution.

The thanks of the meeting were duly accorded to Mr. Siemens for his presentation.

Mr. Richard Aylmer and Mr. W. P. Whitehead were appointed scrutineers of the ballot.

On the motion of the CHAIRMAN, the balance-sheet for the year 1894, of which a copy had previously been posted to each Member and Associate, was taken as read.

The PRESIDENT: I have now to move that the balance-sheet for the year 1894, as just read, be accepted and adopted. I shall be glad to hear if any member has any remark to make thereon. This is the time when any remarks on the finances of the Institution should be brought forward.

Mr. RICHARD AYLMER: I have much pleasure in seconding the motion for the adoption of the balance-sheet.

Sir D. SALOMONS: As your Honorary Treasurer, I think that a few remarks from me may prove welcome before the balance-sheet is adopted.

A few years ago, I called attention to the fact that we were only solvent when the value of our furniture was taken into consideration. That practically meant no surplus whatever.

We stand very differently to-day. For the year 1894 the net receipts were £3,424 15s. 6d., with an expenditure of £2,601; thus there was a cash surplus of £873 15s. 6d.

When we look at the assets for the same year, they were valued at £10,712. In the year 1893 the assets stood at £9,459; consequently there has been an increase of £1,852; in other words, the Institution is gradually growing richer. One circumstance must never be lost sight of: whatever may be the objects of the Institution, it will stand or fall as it is solvent or the reverse.

I would further point out that, even if in estimating the value of our assets the value of all furniture, books, and stock-in-trade —say £1,800—be deducted, you still have assets, in cash and in marketable securities, to the value of nearly £9,000.

I think, therefore, we have a great deal to congratulate ourselves upon. I may mention that since this balance-sheet was made out there has been a further sum of £1,100 invested.

No question having been asked, the motion for the adoption of the balance-sheet was then put to the meeting, and carried *nem. con.*

The PRESIDENT: I will now call upon Mr. W. Langdon to read his paper.

ON THE EMPLOYMENT OF THE ELECTRIC LIGHT FOR RAILWAY PURPOSES.

By W. Langdon, Member.

Mr. Langdon. Having regard to the general character of the title of this paper, the author is desirous it should be understood that the data referred to are the result of experience gained on the Midland Railway. The Midland Railway was by no means the first railway company to employ the electric light for traffic purposes, and the author lays no claim to priority in this respect. On the contrary, he readily recognises the prior claims of others, and he earnestly trusts that his friends and *confrères* will feel no hesitation in bringing forward such information with respect to their own systems as may add to the value of the subject. It is well known that the Great Western Railway, the London and South Western, the Brighton Company, the Lancashire and Yorkshire, the Caledonian, the Manchester Sheffield and Lincoln, as also the Glasgow and South Western, have for some time, more or less, entered upon the employment of the electric light, and that the London and North Western is now applying it to its goods depôts. The most material advance will, however, be found in the central stations recently established, in the one instance by the Great Northern Railway Company, under the guidance of Mr. W. H. Preece, Past-President, at Holloway; and in the other instance that laid down for the Great Eastern Railway at Liverpool Street by our President, Mr. R. E. Crompton.

From all, no doubt can exist that material information is to be obtained—information of value not only with regard to the question of economy, but also in the mode of application. In both the Holloway and Liverpool Street plants we have comprehensive schemes, the product of men of large experience and of the highest ability, from which results of an exceedingly gratifying character may well be anticipated.

The electric light is employed on railways, as in towns, under its two forms, viz., incandescent and arc lighting.

INCANDESCENT LIGHTING.

Mr. Langdon.

Incandescent lighting has so far found its chief employment in the illumination of offices, waiting rooms, and other spaces of limited area. So employed, its claims upon our consideration are those which it has established in relation to our homes—a purer and more healthful light than can be procured from any other illuminant. In offices where a staff of workers are in constant attendance, or where, from the exigencies of the moment, work has to be carried on to a late hour of the night, it becomes to railway companies, as to all other employers of office labour, a factor of importance. To impute to the vitiated air breathed by those who occupy close rooms lighted by gas all the ills to which flesh is heir, would be absurd; still, so long as the air we breathe is robbed of its oxygen and otherwise polluted by the impurities cast into it by whatever may be the source of illumination employed, there can, in point of healthfulness, be no comparison between it and the electric light.

The argument against the electric light for the illumination of offices, waiting rooms, &c., is usually one of expense. With a small installation, or where the current has to be obtained from a supply company, the cost is, at present, in most instances, probably greater than that of gas; but it is open to doubt whether such is really the case when we take into consideration the cleaning and re-decorating of such places. We all know the effect of gas, and the inconvenience of being turned upside down periodically for cleaning purposes. If we save by the electric light but one cleaning in three, it is probable that even the drawbacks of a small installation, or the present cost of current when obtained from an outside source, are fully met.

If to these arguments we add that of health, we have a factor which carries with it far greater weight, and merits far more consideration at our hands. Health is money in more ways than one. It is money to the employer. It is more than money to the employed. It means to the employer more constant attendance, greater energy, and greater devotion to work on the part of the *employé*—gains alike to master and man.

Whether the incandescent light may not yet prove of

Mr. Langdon.

considerable service for the upper floors of goods warehouses, where light is only occasionally required, is an openquestion; and although in such places its advantages in a hygienic sense may be disregarded, it is, with a sufficiently large installation, quite within the realms of economy.

Wherever electric lighting is produced by a local generating plant every increase in the output of current tends to reduce cost. All such additions mean very little additional expenditure in production of current. The first cost is no doubt more than that for gas, but when we reach that point where the annual cost of the light produced is less than that of gas, such extra first cost is soon recovered.

The directors of the Midland Railway Company have only recently installed the incandescent electric light throughout the offices, waiting rooms, &c., of the Derby station and headquarters offices. There is but one opinion of the result. The staff already feel the benefit of it in point of healthfulness and comfort, and the rooms and offices are practically as clean at the present moment as when the light was started.

In the "shipping" offices—offices usually located on the platforms of goods sheds, for the purpose of dealing with invoices, &c., and, as a consequence, necessitating very late, and sometimes all-night, attendance—the advantage of the electric light is still more intensely felt, for in these offices an abundance of light is a necessity.

Arc Lighting.

There is reason to believe that arc lighting is destined to be of great advantage in railway working. In the loading and unloading of stock, and in the marshalling of trains, it is an invaluable adjunct. To be able to unload a train, load its contents into trolleys ready for delivery, and, *vice versâ*, to transfer from the collecting vans to the railway trucks, marshal and despatch them with speed, is economy in capital, men, material, and time—results which are not confined to the depôt at which it originates, but which to a great extent influence the traffic of the entire system.

The advantage of the arc light in goods warehouses, yards,

sidings, and marshalling grounds, presides in the simple fact that it is a larger and truer light than can conveniently be obtained from gas for a similar outlay. By this larger light, work is handled with greater accuracy and greater despatch. To be able to load a given number of trains in, say, three-fourths the time formerly occupied, is tantamount to increasing the capacity of the premises, warehouses, yard, &c., 25 per cent.; or, supposing a depôt required enlarging in order to meet an increased traffic, the introduction of the electric light should meet the contingency, and avoid purchase of land, erection of additional buildings, &c. In like manner, with new buildings, economy in time means economy in capital outlay: the buildings need not be so large, because the traffic can be disposed of in less time. Mr. Langdon.

Probably to no part of a railway system is the electric light of greater utility, or more valued, than in yard shunting, or the marshalling ground. With a brilliant light at the junction points the marshalling process may be carried on with almost the same despatch and certainty as during broad daylight. Risk of life and limb is largely reduced, whilst it has been found to be in no small degree useful in preventing that petty pilfering to which the absence of light is often an inducement. The author believes it to be a fact that the first arc lighting plant installed for yard working—viz., that at Nine Elms, which was devised by Mr. W. H. Preece, Past-President, and installed by Mr. R. E. Crompton, our present President—was laid down mainly with a view to suppress certain depredations on valuable goods which the absence of light appeared to foster, and upon which its introduction has had so marked an effect as to help largely to defray the cost of the installation.

Although, as already stated, by no means the first to employ the electric light, the Midland Railway was not long in recognising its advantages. In 1889 its adoption was recommended by Mr. G. H. Turner, the company's general manager, who at that time filled the post of goods manager, and who recognised in it one of those aids towards the rapid handling of material which at that time so largely occupied his attention in relation to the remodelling of many of the company's goods stations. In

Mr. Langdon

this remodelling of their chief goods depôts the directors have called to their aid all the most modern improvements, and amongst these that of the electric light. There is nothing of a purely novel character in the application of the electric light to these improvements, and the author feels that only in illustration of its general application and usefulness under aspects perhaps slightly varying from those which are associated with it in its use for public street lighting, can this paper lay claim to consideration.

The first electric lighting plant laid down on the Midland was that established for lighting the St. Pancras Hotel. The generating plant established for this service is in duplicate, and consists of a simple non-compound horizontal steam engine and two dynamos, each set being of 28-kilowatt capacity. The number of lights in the hotel is equal to 1,330 8-candle-power lamps at 110 volts, and the maximum load reached has been 320 amperes.

The light load is met by accumulator cells; the dynamos being so arranged that one of the two driven by each engine can be employed for charging them, while the other is furnishing current direct to the lamps.

The first arc lighting plant laid down was that experimentally installed by Messrs. Laing Wharton & Down, as representatives of the Thomson-Houston system, in 1888, at St. Pancras. The lighting was confined to the passenger station. In 1890 this plant was purchased by the Midland and extended to the St. Pancras goods station and yard. Insulated overhead wires, partly okonite, partly vulcanised, were employed. The installation has from time to time been largely extended, and now consists of 242 2,000-candle-power (nominal) arc lights, employed on the St. Pancras passenger station, the St. Pancras goods station and yard, the Somers Town (or New St. Pancras Goods) warehouse and yard, sidings, and approaches.

The engines employed for this installation consist of two pairs of simple horizontal non-compound steam engines, coupled, served by locomotive boilers, three 50-light T.H. dynamos being worked from countershafting driven by each pair of engines. This countershafting was originally arranged so that it might be coupled for cross driving if needed; but up to the present no

such need has presented itself. The engines were made and placed in position by Mr. Sam. Johnson, the company's locomotive engineer, and it is impossible to speak too highly of their performance. Mr. Langdon.

The electric lighting plant now in use or under construction on the Midland is approximately as represented in

Table I.

Engine B.H.P.		Locality.	Arc Lights.		Incandescent. 8-C.P.
Steam.	Gas.		2,000-C.P.	1,250-C.P.	
200	90	Bradford	...	190	1,100
190.	...	Leeds (Hunslet Goods Depôt)...	...	150	523
190	...	Sheffield (Wicker Goods Depôt)	114	...	300
190	...	Liverpool (Sandon Dock) ...	...	126	100
190	25	Birmingham (Central Goods) ...	73	...	435
300	..	„ (Lawley Street) ...	...	200	400
320	...	Derby	...	...	4,500
...	250	Leicester (Passenger and Goods)	160	...	400
300	...	St. Pancras „ „	242	...	...
...	...	„ Hotel	...	...	1,330
200	...	Nottingham	55	75	300
..	40	Wellingboro'	35	...	100
2,080	405		679	741	9,488
2,485			1,420		

Steam may be said to be employed throughout, with the exception of Leicester and Wellingboro'. Leicester—which will be dealt with more in detail later on—and Wellingboro' are dependent upon gas. A small gas engine is also employed at Bradford and Birmingham (Central) for the light load; and, owing to want of space for extension of boilers at Bradford, we are now laying down two 40-H.P. gas engines to meet an extension of arc lighting there.

The boilers are in all cases of the locomotive type.

The type of steam engine varies.

Mr. Langdon.

At Bradford some 200 H.P. of the engines are identical with those previously described as in use at St. Pancras.

At Birmingham (Central Goods, and Lawley Street Goods), and at Sandon Dock, Liverpool, horizontal two-cylinder simple engines by Davey Paxman are in use.

At Leeds, two-cylinder simple by Fowler; at Sheffield, single-cylinder steam jacketed by Marshall; at Derby, Willans & Robinson single-acting compound,—all are non-condensing.

The Willans & Robinson engines at Derby drive direct; and, with the exception of St. Pancras and Bradford, where counter-shafting is employed, the driving is by belting direct from the engines. The engines employed for the arc lighting are generally 80-H.P., and are provided with two fly-wheels, each driving an arc dynamo.

At Leicester, with the object of testing the economy, or otherwise, of a gas-driven plant as against steam, a gas plant has been laid down. It has only recently been brought into use, and the installation is not even yet complete, the new passenger station being still in progress. A Dowson gas plant has been installed, and connection with the town gas has also been provided. The valves in the supply pipes are so constructed that either form of gas can be laid on to the engines without stoppage of the latter.

The generating plant consists of four 50-H.P. gas engines for driving three arc machines and one low-tension machine, and two of 25 H.P. for two small incandescent dynamos.

Arc Lighting System.

Throughout the service the arc lighting employed in the yards and shunting grounds is on the series system. In certain of the goods sheds the series-parallel—two lights in series—system is used.

Brush series machines are in use at Leicester; the Thomson-Houston at all other points.

Incandescent System.

The machines laid down for incandescent lighting are from

various makers—Siemens, Edison, Hopkinson, Elwell-Parker, and Holmes. Mr. Langdon.

The incandescent lighting, with the exception of Derby, is on the simple parallel system. At Derby the three-wire system, with feeders, is in use. It will be seen from the skeleton plan (Plate A, Fig. 1) that the blocks of buildings to be lighted are somewhat scattered, and that the generating station itself is situated some distance away from the lighting. It is practically a small supply station, with, at present, a very bad load-factor (Fig. 2)—much worse than that of a town supply station.

DERBY. *Diagram of Max: and Min: Load.*

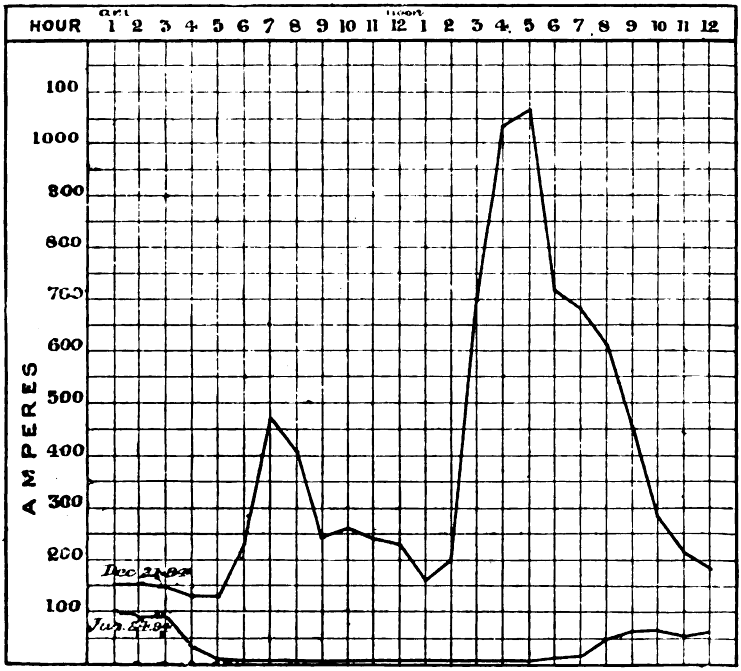


Fig. 2.

Here, as also at Bradford, Sayers's automatic compensators are employed to counteract the fall of potential as the current increases.

The Midland Bradford Station, as is perhaps well known, is

Mr. Langdon. electrically lighted throughout, with the exception of a few gas lights under some low awnings at the distant ends of the platforms. Originally the incandescent lighting was provided for by compounded machines in duplicate. To change over from the working to the duplicate set, involved careful handling on the part of the attendant. Discussing the matter with Mr. J. Sayers, my assistant, under whose supervision Bradford came, it was decided to insert a series machine in one main and to dispense with the compounding. In the end this resulted in the employment of Sayers's "compensator," which, I believe, was the first self-exciting machine made without winding on magnets. The application of this machine has been attended with the most perfect success. The transference of the load from the active to the duplicate set is now simply that from one shunt machine to another, while the compensation of the loss in pressure consequent upon the increase in current is most perfectly met.

At Bradford the wiring is on the simple parallel system. At Derby Dr. Hopkinson's three-wire system is in use. The success attending the employment of Sayers's compensators at Bradford naturally suggested their employment at Derby. At Bradford the extent of the compensation is limited to 11 volts, with a maximum current of 320 amperes. At Derby we have to serve a network, and the compensation is more involved, owing to the loss in the middle wire when either of the two sets of three separate groups of feeders is unevenly balanced. Dealing with one set, the arrangement is as follows:—The outside wires of the grouped feeders are connected through a series machine driven by motor at a constant speed of 820 revolutions. This compensates for the loss in each outside wire.

The loss in the middle wire is, of course, double-acting, raising the volts on the light load side and depressing them on the other. To meet this, Mr. Sayers takes the middle wire round the magnets of each of the two series dynamos, but in opposite directions one to the other. Any current passing in this wire therefore adds volts to one, and decreases the volts in the other, equally; and by suitably proportioning this winding it can be made to practically neutralise the loss in the third wire.

If both sides are fully loaded, and there is no loss, or no current passing in the middle wire, each series machine has to raise the voltage, on a current of 350 amperes in each, some 7 volts; but, as the third wire is one-half the section of the outer wires, it follows that, if one side should be fully loaded and the other not loaded at all, the compensators have to give 7 + 14 = 21 volts, and this they should do under the arrangement provided. Mr. Langdon

Such an extreme case never has arisen in practice; still, serious differences do occur—more so, perhaps, in such a supply as that under reference than in a town service—for the reason that, however equally you may plot out your scheme, it is quite impossible to foresee all those contingencies which, calling for extra attendance on the part of a special branch of the service, call for the load on that side accordingly.

A further advantage to be derived from the use of these compensators is to be found in the regular range of speed at which the engines require to be worked between light and full load. The volts of the main dynamos only require to be kept at the normal pressure—say 110. There is no need for running the speed up so as to increase voltage to meet the drop consequent upon increase of current. It is one of those arrangements which simplify the working of a station at moments of pressure.

Installation.

The establishment of the various installations, with the exception of that originally laid down by Messrs. Laing Wharton & Down at St. Pancras, has been carried out by the company's own staff, under the immediate supervision of the author and his assistants, Mr. W. L. Preece and Mr. J. Sayers; and no small portion of the fittings required have been made in the company's electrical workshops.

Arc lighting wires not employed for the interior of buildings are laid underground. When drawn into iron piping, the wire is not armoured, but is protected by an extra outer serving of flax, hemp, &c. When armoured wire is used it is covered by ordinary drain tiles, split, or by planks of wood, employed more with the object of indicating to those who may have occasion to

tr. angdon. disturb the ground in its neighbourhood the fact that there is something beneath it, than with any idea of its power to absolutely resist injury. The insulation of the cable thus employed is specified at 5,000 megohms.

Arc lighting wires inside buildings are insulated with vulcanised india-rubber to not less than 300 megohms, and are, as a rule, supported on Johnson & Phillips's oil insulators. All suspended lamps are also insulated from the suspension fixing by oil insulators made for the purpose by the same firm.

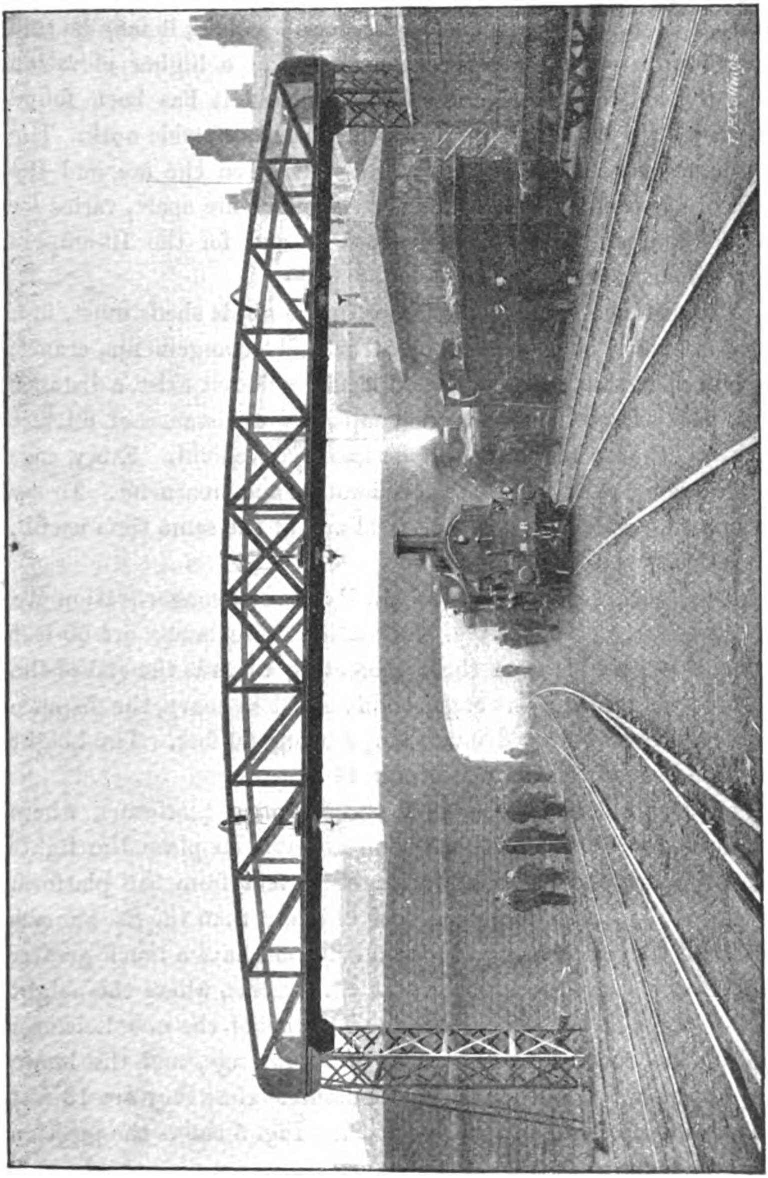
For goods yard and siding working the selection of the positions for the lamp pillars, and the height the lamp should stand from the ground, are matters of importance. The former is only to be dealt with in a satisfactory manner by learning from those who have the handling of the work, and by personal observation, where the light is most needed.

Each group of points requires special consideration. Shunters should be able to see when waggons have passed over each point, so that they may readily "knock off" the next waggon or group of waggons as the required points are clear of the trucks previously despatched over them.

At the mouth of the Leeds goods yard, where there are four roads, a light girder bridge (Fig. 3) has been thrown across, from which three lights are suspended between the roads, and the lighting is in consequence very good. The point is an important one, as from each line several fans of roads radiate, and it is beneath this bridge that the main part of the shunting is done. Without the girder bridge it would have been impossible to adequately light the intervening spaces between the roads.

It will be noticed that two classes of lamps are in use—that affording what is termed 1,200 C.P., and that known as 2,000 C P. The former requiring a current of 6·8 amperes, and the latter 10 amperes, the 10-ampere lamp will afford a larger body of light than will the 6·8-ampere lamp In station buildings, where structural arrangements do not intervene, the 10-ampere lamp illumines a larger space than that produced by the lesser current, and is, as a rule, arranged at a higher elevation, and further apart in consequence. In goods yards and open spaces,

Mr.
Langdon.



Mr. Langdon.

theoretically, they should illuminate an equal extent of area, In point of fact, however, the illumination does not bear the same relation as when within enclosed spaces. Again, it may be said the 10-ampere lamp should be arranged at a higher elevation than the smaller lamp, but on the Midland it has been found convenient to employ only one class of pillar for outside work. The height adopted is 20 feet—*i.e*, 20 feet between the arc and the level of the rails. The distance these pillars are apart, varies for the 6·8-ampere lights from 90 to 100, and for the 10-ampere lights from 100 to 120 feet.

The distance between the lamps in the goods sheds must, in a great measure, be controlled by structural arrangements, cranes, &c. In open sheds, where these difficulties do not exist, a distance of 45 feet between 6·8-ampere lamps, and a distance of 60 feet between 10-ampere lamps, will be found convenient. Every case will, however, call for special consideration and treatment. To lay down a rule which shall be universal and at the same time useful, is practically impossible.

The lights employed on the St. Pancras passenger station are 10-ampere. At the head of the station those lamps are 60 feet apart, the space between them increasing towards the end of the platforms, where the passenger traffic is not so heavy, the distance between the two most distant lamps being 90 feet. The height of these lamps from the platform is 14 feet.

On the Leicester passenger station main platforms, where 10-ampere lights also are used, we are able to place the lights 90 feet apart, and at an elevation of 15 feet from the platform level. At Leicester the glass roof is lower than the St. Pancras roof, and the walls, being of glazed material, have a much greater reflecting power than is the case at St. Pancras, where the height of the lamps is 14 feet. The side platforms of the new Leicester station are covered by the usual glass awnings, and the lamps have to be arranged to meet the structure. Here they are 13 feet above the platform, and 75 feet apart. Fig. 5 shows the position of these lamps.

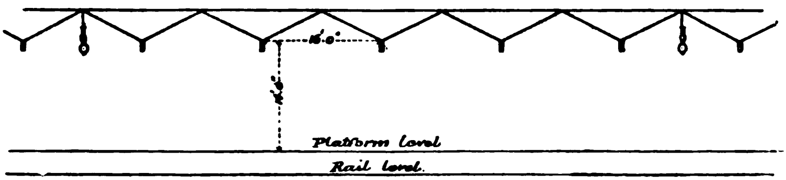
Plate B, Fig. 4, is a plan showing the positions of the lights in the Birmingham (Lawley Street) goods depôt and yard. The lights

employed are 6·8-ampere. This plan will probably afford a better idea of the disposition of the lights for such yards and buildings than will any written description. The positions of the lamps Mr. Langdon.

— LEICESTER —

— Longitudinal section of awning on the side platforms showing position of Arc lamps. —

Fig: 5



— Scale. 24 feet to an inch. —

are shown by stars. Within the warehouses the cranes, with their radii, are represented by broken circles. The Lawley Street depôt is still in an incomplete condition. A range of offices—now about to be constructed—will occupy the Lawley Street frontage, and the space between these offices and the main warehouse will be covered in by an awning. The lamps shown in this space are there temporarily to meet present demands only.

The lamps employed are double carbon, jointly affording 16 hours' lighting. 13-mm. carbons are used for the 10-ampere lamps, and 12-mm. for the 6·8-ampere lamps, for both top and bottom carbons.

In the earlier plants laid down the lamps are protected by sheet-iron hoods, A (Fig. 6). The lamp is suspended from a cross-piece of timber, B, by insulated hooks, or hangers, C, and it is further supported at its base by an insulator, D.

Later constructions have been equipped with four-sided lanterns (Fig. 7). The top portion of the lamp is insulated from the lantern at A by a block of dry wood and insulated hooks, and the base, as in Fig. 6, is supported on an insulator, B.

Mr. Langdon.

These lanterns have been glazed—

(*a*) With plain clear glass,
(*b*) With what is termed "blurred" glass,
(*c*) With fluted glass,

samples of which are on the table.

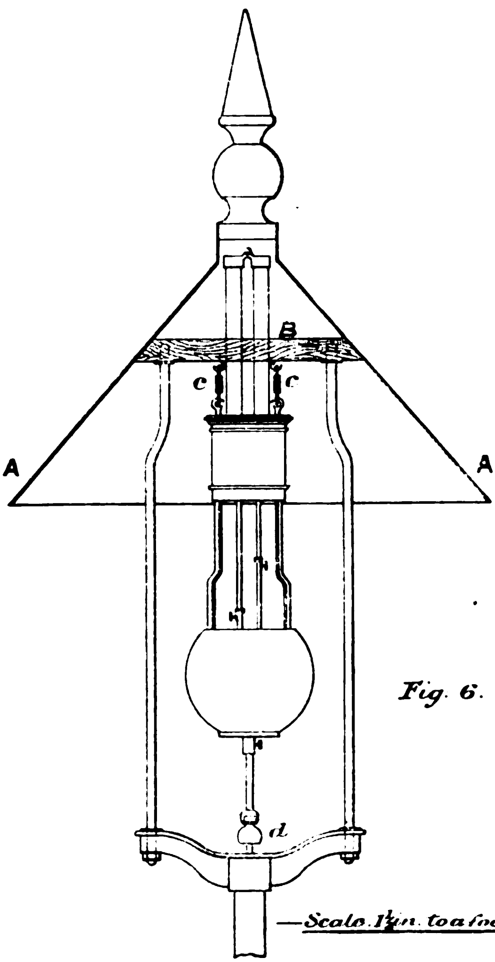


Fig. 6.

—Scale 1¼ in. to a foot.—

The clear glass can be used only to a limited extent. The naked arc is objectionable to men working within its immediate range. Clear glass is perhaps still more objectionable, from the strong shadows which attend its use. "Blurred" glass has been

employed with the object of toning down the glare of the arc and Mr. Langdon.
breaking up the shadows. It is not, however, in this respect so successful as the fluted glass. With the flutings horizontally arranged the shadows are completely broken up, and the refraction is so good that the light immediately underneath the pillar is practically as good as that 10 feet distant.

The lanterns are made so that the chimney may lift off at A. All lamp pillars are provided at their base with switches which entirely sever the connection of the lamp wires with the line wires; and in all modern constructions the switch, when in the "Off" position, also places the lamp to earth. The lamp pillar complete is shown in Fig. 8. The switch is enclosed in the base of the pillar.

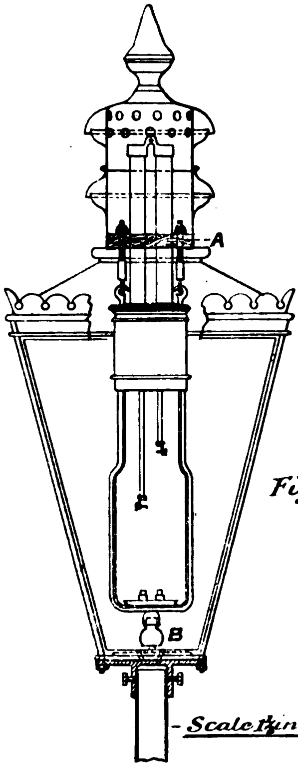


Fig: 7.

- Scale 1½in to a foot. --

Where lamps are worked in series it will, in determining the circuit arrangements, be found desirable to ascertain the period during which the lights are required for use, and to group them accordingly, so that they may be turned off and on in sections, if possible, from the switch-board in the dynamo or transformer room. The groups should also be so arranged that, combined, they may equal the capacity of a machine. The circuits applicable to indoor lighting should be so arranged as to admit of their being worked independent of the outdoor or yard circuits, the light being required indoors at an earlier hour than those out of doors.

Mr. Langdon.

For incandescent work the wires are, as a rule, run in grooved casing of the orthodox kind. The casing and cover are fixed by the outer and not by the centre fillet; the object in rejecting the centre screw being to exclude the aid it would afford to any *creeping* of the current across the centre fillet, should leakage at any time arise. Under such a condition it is clear the screw in the centre fillet would largely reduce the resistance between the two wires—that it would, in fact, become a sort of "half-way house" towards the passage of the current, and tend to ignite the wood. The wooden casing as at present employed affords a convenient mode of dealing with the wiring of rooms; but, in view of the description of casing which one not unfrequently sees employed, and, indeed, under any circumstances, it can hardly be regarded as a perfectly satisfactory mode.

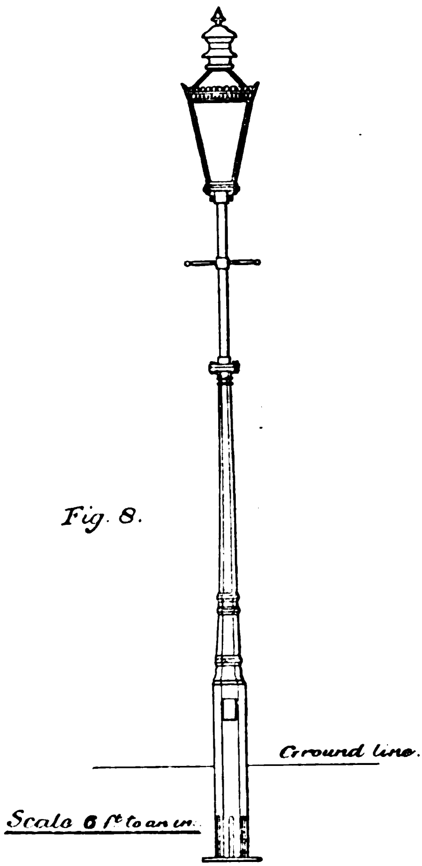


Fig. 8.

If an artistic rectangular casing or tubing of metal which would not readily melt could be produced at a reasonable price, it would no doubt prove exceedingly serviceable.

Apart from the points referred to, there is, in the application of the electric light to railway working, little of a sufficiently special character to call for further reference. Numbers of the fittings employed, switches, switch-boards, controlling arrangements, &c.—most of which are made in the company's own shops—

are to be found in connection with each installation; but in this the installations on the Midland do not differ from all other installations, inasmuch as most have one or more specialities peculiar to themselves. Any such specialities are best investigated on the spot, and the Midland Railway Company will at all times be pleased to afford facilities for an inspection of any one of their works. Mr. Langdon.

The question which will probably most interest railway companies is that of

COST.

To lay down an electric lighting plant is probably more costly than to establish means for illumination by gas; but when we take into consideration the relative cost of working, the light derived from each source—light for light, or candle-power for candle-power—the advantage is so largely in favour of the electric light as to more than fully compensate for any larger first outlay which may be incurred.

The tabulated statement (II.) will afford data with respect to the output and cost of working of several of the electric lighting stations on the Midland. Certain charges for labour, engine and boiler repairs, and for coal, owing to charges for these items being combined with similar charges for hydraulic machinery located in the same building, it is possible, are not so strictly allocated as is desirable for a paper of this kind; still they are not largely inaccurate. They should, however, be regarded as maximum charges.

Nor are the charges in any case comparable with those of city supply stations. The arc lighting load, varying throughout the night according to traffic demands, is not so constant as that of of a town service; while that for incandescent lighting lacks the continued evening service associated with town lighting. At goods stations and for office lighting the latter rises to its maximum in the depth of winter for a period not exceeding an hour and a half daily, viz., 4 to 5.30 p.m.; while in summer the demand is still less. Still, the result, as compared with the cost of current obtained from a public supply source, is greatly in favour of an independent generating plant.

Mr. Langdon.

Table II.

MIDLAND RAILWAY.—ELECTRIC LIGHTING.

Details of Cost, Half-Years ending June 30th and December 31st, 1894, including Maintenance and Renewal.

Installation.		Total Units.	Total Cost.			Cost. Per Unit, including all Charges.	Per Arc Lamp Hour.	Per Unit for Incandescent Lighting.	Per Unit for Labour.	Per Unit for Material.	Per Unit for Repairs.	Per Unit for Coal.
			£	s.	d.	d.	d.	d.	d.	d.	d.	d.
Somers Town	Half-year ending June 30	170,171	2,294	5	10	3·23	1·61*	—	1·15	0·65	0·29	1·00
	" " Dec. 31	187,326	2,506	6	4	3·21	1·60*	—	1·09	1·03	0·27	0·82
Bradford	" " June 30	82,984	1,115	15	0	3·23	1·39	2·7	1·19	0·56	0·47	0·80
	" " Dec. 31	100,388	967	5	6	2·31	0·98	2·0	1·06	0·56	0·18	0·44
Birmingham (Central) ...	" " June 30	70,774	950	10	11	3·22	1·60*	3·3	1·43	0·68	0·35	0·48
	" " Dec. 31	90,847	945	4	6	2·49	1·22*	2·7	1·03	0·64	0·34	0·37
" (Lawley St.)	" " June 30	89,960	1,113	9	5	2·97	1·00	—	1·33	0·80	0·20	0·60
	" " Dec. 31	102,481	1,103	14	2	2·58	0·85	—	1·25	0·62	0·24	0·44
Hunslet	" " June 30	70,985	1,157	4	7	3·91	1·41	3·6	1·80	0·91	0·27	0·79
	" " Dec. 31	75,864	1,237	6	1	3·91	1·47	4·0	1·89	0·85	0·55	0·60
Derby	" " June 30	60,389	873	8	9	3·47	—	3·4	1·70	0·76	0·19	0·65
	" " Dec. 31	77,740	1,082	14	9	3·34	—	3·3	1·45	0·94	0·40	0·53

* These are 10-ampere lamps, 2,000 C.P. All others are 6·8-ampere, 1,200 C.P.

Mr. Langdon

In criticising Table II.—viz., that of cost—we must also bear in mind the fact that the cost per unit carries with it the cost of carbons and carboning the arc lamps, together with cleaning, repairing, &c.; and that the cost for incandescent lighting also, as a rule, covers the renewal of lamps. The only exception rests with Bradford, where the lamps in the hotel are replaced by the hotel department, and their cost is not therefore included in the cost shown in the tabulated statement.

Again, it should be understood that the appropriation of charges to incandescent and arc lighting, where both are in operation, is unavoidably somewhat arbitrary. There are many items of cost which apply to both, such as labour in attendance upon the machinery, cleaning, &c. These have to be divided and appropriated as is considered fair and reasonable. The question will arise, Is one favoured at the cost of the other? The result must speak for itself.

Still one more condition! The output is obtained from records taken hourly, not from meter. The data are not, therefore, absolutely correct; but, taken over a period of 12 months, they are sufficiently reliable to afford a fair index of the cost.

Dealing with the table as it stands, we find the cost for arc lighting as under:—

Depôt.	Cost per Lamp per Hour.
	d.
St. Pancras (Somers Town), 2,000-C.P. lamps	1·61
Birmingham (Central Goods), 2,000-C.P. lamps	1·41
„ (Lawley Street), 1,200-C.P. lamps	0·93
Leeds (Hunslet), 1,200-C.P. lamps	1·44
Bradford, 1,200-C.P. lamps	1·19
Total	6·58
Average	1·316

The lighting at Liverpool (Sandon Dock) and Sheffield has so

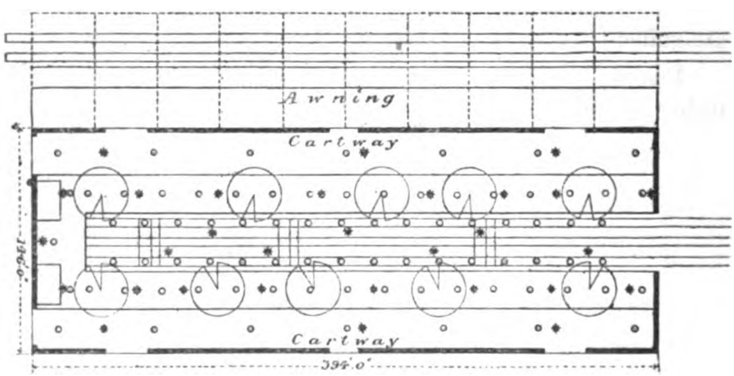
Mr. Langdon.

recently been brought into use that returns from those depôts are purposely omitted. That at Wellingboro' has only now started, and that at Nottingham is under construction.

The daylight output for incandescent lighting at all of the arc lighting depôts is small. At Birmingham (Central), where the lights are required in the bonding stores throughout the day, and at Bradford, where the demand applies to the hotel, station offices, and waiting rooms, this is met by a gas engine.

Some of the recently constructed goods depôts afford an opportunity for comparing the cost of the electric light with that of gas. Gas has been adopted as an alternative form of light in case of failure with the electric light. Consequently, the gas lighting is what may justly be regarded as modern gas lighting, affording an amount of light which, though not nearly so great as that afforded by the electric light, is greater than that which attained, and still attains, under the old system of gas lighting.

Fig: 9.



— M. R. BIRMINGHAM CENTRAL GOODS SHED —

— Plan showing distribution of Gas & Arc Lamps. —

— Scale. 120 feet to an inch —

Arc lamps ◆
Gas lamps o

Fig. 9, which is a plan of one of the warehouses, shows the positions of these gas lights, together with those of the electric lights. The former are on the regenerative principle, and are

each nominally 150 candle-power; the latter are arc lights, nominally of 2,000 candle-power. Mr. Langdon.

It is officially stated that 5 cubic feet of gas, burnt under the principle referred to, will produce 25-candle-power light for one hour. There are 81 150-candle-power gas lights. The price of gas is 2s. 3d. per 1,000 cubic feet, less 5 per cent. On this basis, the cost of gas lighting would be 62d. per hour.

There are 30 arc lights, the cost of which, as per Table II., averaged 1·41d. per light per hour. The total cost per hour would therefore be 42·3d., as against 62d. for gas; the relative nominal candle-power being—

Gas	12,300
Electric light	56,000

At Lawley Street, Birmingham, there are 53 arcs of 1,200 C.P., and, as an alternative, if required, 130 gas lights of 150 C.P. each. The arcs here average a cost of 0·93 pence per lamp per hour. Gas is 2s. 3d. per 1,000 cubic feet, less 5 per cent. The result works out per hour—

53 arcs, affording 63,600 C.P.	49·29 pence.
130 gas, affording 19,500 C.P.	100·00 „

At Somers Town (New St. Pancras Goods) the gas used is manufactured at the company's own gas works at Brent, near "Welsh Harp," and is priced at 2s. per 1,000 cubic feet. The result is as follows:—

29 arc lights, affording 58,000 C.P., cost per hour, 47 pence.

86 gas lights, affording 12,900 C.P., cost per hour, 61 „

If we extend these results so as to cover a period of a year—say 3,000 hours—we have a saving of £1,050 per annum on the three ground floors of the goods warehouses referred to; and it is important to observe that, whereas the charges for the electric light cover all charges—repairs, cleaning, &c.—the charge for gas is for gas *pure and simple:* there is no provision for cleaning or for repairs.

Gas Plant.

In the early portion of the paper reference has been made to a gas plant recently laid down at Leicester. This plant (Plate C, Fig. 10) consists of four Crossley gas engines, required to deliver

Mr. Langdon.

under Dowson gas 40 H.P., and under coal gas 50 H.P., on the pulley of the dynamo, for the purpose of driving three 50-light 2,000-C.P. Brush series high-tension dynamos, and one Brush low-tension dynamo employed chiefly for arc lighting; and two 14-H.P. nominal Crossley gas engines, required to transmit 25 H.P. to the pulleys of two Siemens incandescent machines.

The service pipes are in duplicate; that is, one is in connection with the town gas, the other in connection with a Dowson gas plant. The engines are provided with valves which admit of the gas supply being changed over at will from coal gas to Dowson, or *vice versâ*.

The object of this arrangement is to test the cost of gas as against that of steam.

In construction, an ordinary gas plant saves the cost of boilers and chimney shaft, and occupies less space, thereby effecting an economy in cost of building and cost of land. In maintenance there ought to be a saving in labour: no stoking is required when working off coal gas; and, as a rule, the attendance upon engines and dynamos is shared by the electrical attendants—engine-drivers and stokers are non-existent. Thus it would appear that there should, where coal gas only is used, be a saving of from two to three men.

With a Dowson plant more space is occupied than if coal gas only is relied upon; and a man is also required for stoking and attendance upon generators. It remains to be seen if sufficient economy attaches to the employment of Dowson gas to compensate for the additional cost of plant, space, buildings; and as regards maintenance, repairs, and labour.

The experience so far gained is scarcely sufficient to justify a definite expression of opinion, but the following test figures may be of some service. They should be accepted with reserve.

The test extended over a period of four weeks during December, 1894, and it shows the following to be the cost of running, exclusive of repairs or renewals, but inclusive of carbons.

Dowson gas was used during week-days, and coal gas on Sundays, or very occasionally.

Arc lights, 2,000-C.P. ...	1·2 pence per lamp-hour.
Incandescent lights ...	2·3 pence per unit.

It should be added that, although the output is not at its maximum—the whole of the proposed lighting not being yet established—the result here given is for a busy period. Mr. Langdon.

There will be periods during which the output will be much less, and consequently the result per lamp-hour not so good.

As suggested, it is somewhat premature to attempt to draw a reliable comparison between this cost and that resulting from steam power; still, the figures are such as to inspire confidence in the experiment. At some future date the author will be glad to communicate the result of further experience.

Conclusion.

It is submitted that the experience so far gained establishes beyond doubt advantages largely in favour of the electric light as an illuminant for dealing with railway work wherever a large body of light is required.

At the same time, we must be sensible of the fact that daily experience teaches us to look for further developments in the machinery employed; and although this does not necessarily entail replacement of existing plant, it is as well to mark well that which it is desirable should be achieved. One improvement should be the abolition of belt-driving. All driving should be direct from the shaft of the engine.

The result will be less danger of interruption, less space, greater regularity. The speed of dynamo machines for arc lighting in series is now being brought within the compass of high-speed steam engines. This accomplished, the propriety of direct driving for series, as well as series-parallel-arc commends itself to our consideration.

A further advance is to be looked for in the unification of the class of generator. At the present moment it is necessary, where series-arc and incandescent lighting has to be provided, to lay down independent power and independent machines for each, or to drive the arc lights from alternating or transformed alternating currents. To drive arc lights on the series-parallel system for any material distance from the generating plant would entail so large an outlay in cable as to strongly militate against its adoption for such small plants as railway companies require.

Mr. Langdon. It would appear that what is needed is a generator the energy derived from which may be applied alike to incandescent lighting, arc lighting, or motive power, for the latter is an application which will have to be provided for. Is this to be most economically and most successfully met by an alternator or by a direct-current machine? Are we to look for help in this direction from "rectified" currents.

If a system having for its basis one common generator can be established without too great a sacrifice in efficiency, small installations, such as would be of great service to railway companies in many busy centres, could then be brought within a more economical compass than is now the case. Three units—one for the light load, another for the full load, and a third of a similar capacity as spare—would probably meet the requirements of every depôt; whereas, at present, spare units for both arc and incandescent work are necessary.

For instance, at Birmingham (Lawley Street) the plant, when all is complete, will consist of six 50-light arc lighting machines and two incandescent machines. Now, if all this could be met by three generators—one for the light load, arcs and incandescents combined, another for the full load, and a third as spare—the engine and dynamo space would be largely reduced, and labour and repairs correspondingly lessened.

As opposed to this would be the cost of transformers and transforming rooms. To help this we might possibly have reduced cost in cables; so that, regarded as a whole, provided that which is sought can be secured without too great a sacrifice in efficiency between the generative output and the production of light, or other means of consuming the energy, there would appear to be reason in regarding such an arrangement as highly favourable to economy.

Few, probably, will doubt that electrical energy is destined—and that at no distant date—to become a useful and economic agent at all important railway centres, not merely for lighting, but for traction and other purposes at present met by horse labour, hydraulic power, &c. In determining the ultimate design of generator and mode of working, this probable demand should

be present to our minds, both with regard to the electrical system and the design of buildings for generating plant. That the demand for electrical energy will increase we may be sure, and our provision should in all cases be such as will admit of extension. Mr. Langdon.

Attached to this paper will be found copies of some rules and regulations in relation to restrictions, duties, and the management of electrical depôts such as are required for use on railways, which experience has dictated, and which may possibly be of service to others. Appended are also some precautionary instructions which have been issued for the avoidance and treatment of accidental shock from electrical current.

APPENDIX.

Useful Rules for the Guidance of Attendants and others engaged in Electric Lighting Duties.

1. It is important that everyone who has any duty to perform in connection with electric lighting machinery and apparatus should bear in mind that the lighting of those buildings, yards, &c., to which it is applied is dependent thereon, and that the apparatus employed for the purpose must, as far as is possible, at all times be ready for use.

2. The foreman of every installation, or, in his absence, the leading-man, must on the approach of fog, or darkness from other cause, get the machinery into motion, and the lighting in operation as soon as possible, after satisfying himself that the lamps on the circuits are ready for use.

3. Where the lighting of the station, buildings, &c., is entirely dependent upon the electrical machinery, steam must be in readiness for use at any moment.

4. All machines, wearing parts, belts, circuits, &c., are to be carefully tested and examined when shut down, so that any defect may be at once rectified.

5. One hour prior to the usual time for starting, the attendants in charge of engines and dynamos must carefully test and examine all parts of the machinery, circuits, &c., under their charge, and

Mr. Langdon. satisfy themselves that all is in good working trim and capable of carrying on the run for the time required. If any doubt should arise, the machinery must be started at once, in order that its condition may be practically tested.

6. Should anything transpire which may prevent the lighting, immediate notice must be given to all whom the absence of the light will affect, and particulars of the cause must be reported by telegraph to the divisional superintendent.

7. When necessary to change over from one set of machinery to another, or from one machine to another, every effort must be made to effect the change without interrupting the lighting more than is absolutely unavoidable.

8. The foreman in charge of the installation will be held responsible that a sufficient stock—not less than one month's supply—of oils, carbons, and all other necessary stores, is kept on hand.

9. The inspector or foreman [or, in his absence, the senior man in charge] will be held responsible for the due and efficient performance of the duties of his staff. He is required to appoint the duties and rounds of the dynamo attendants, trimmers, cleaners, &c.; to see that those duties are faithfully and strictly carried out; that all parts of the apparatus are kept in perfect working order; to take every step in his power to ensure the successful working of the machinery and apparatus entrusted to his care; and to bring under the notice of his superior officer without delay any neglect of duty.

10. Smoking when on duty is strictly prohibited.

11. Attendants, trimmers, and others are required to sign on and off in the duty book provided for that purpose.

12. Hot carbons when removed from a lamp are to be placed in a receptacle safe from fire. Any man throwing a hot carbon to the ground will render himself liable to instant dismissal. The ends of all burnt carbons must be collected and brought back to the engine room.

13. Foremen and trimmers are to satisfy themselves that no possibility exists of burning particles falling from or passing out of the lamps.

14. Open lights are not to be used in goods-sheds, depôts, or elsewhere, without special sanction from the senior officer of the installation. Mr. Langdon.

15. All lamps employed in goods warehouses, sheds, or other covered spaces or rooms, are to be taken down and thoroughly overhauled at periods not exceeding six months in duration.

16. Whenever steps or ladders are required for use in public roadways, sidings, or lines of railway, every precaution in order to avoid accident by or to passing vehicles or foot passengers is to be adopted. If necessary, a man is to be posted at the foot of the steps or ladder to protect the man standing thereon, and warn approaching foot passengers or vehicles.

17. All men employed, whether trimmers or others, are required to take every precaution to avoid the possibility of accident to themselves or others. Anyone who may be called upon to deal with apparatus through which the electric current is at the time passing, or during such time as there may be a risk of the current prevailing, is to use sound india-rubber gloves.

18. Each trimmer, on completing his section, is to report the same personally, or by telephone, where such is provided, to the engine room.

19. No circuit is to be brought into operation for lighting or other purposes until it has been ascertained that the trimmers and all workmen are clear from it.

20. Workmen, when going off or coming on duty, are required to leave the premises by the route indicated by the officer in charge of the installation,—and which is to be that route which will afford the most proper means of reaching the public street or roadway without crossing working lines of railway.

21. Disregard of instructions, neglect of duty, or incivility, will in all cases be severely dealt with.

Dynamo Room.

22. The dynamo room must always be clean, and everything in its place ready for use.

23. Immediately a machine is shut down, it must be cleaned and prepared for further use in case required, and covered up till needed.

Mr. Langdon.

24. All machines, circuits, and parts of apparatus must be tested when shut down, and an hour before being started. Where incandescent machines are kept running for several hours, the machine, as also the circuit, may be tested for earth during the run; but such tests are in all cases to be made through a "resistance" approved for the purpose by the superintendent, and as may be directed by him.

25. Where the frame of a high-potential machine is insulated from the bed-plate, daily tests are to be made in order to ascertain if there exists any leakage from the coils or other parts of the machine to the frame from which it is insulated. If such is found to exist, those attending it are to be personally warned, and a notice in writing affixed to the machine, cautioning all whom it may concern. The cause of the leakage should be traced, and removed with all possible despatch.

26. Any difficulty which may present itself,—loss of insulation of wires, or in the machine,—is to be immediately reported to the superintendent.

27. Before starting, all binding screws and bolts should be examined. If a binding screw is loose, its surface should be examined. If found to be burnished, a fresh surface should be provided.

28. A diary and a duty book are to be kept at each depôt. In the former is to be entered, by the chief attendant in charge, the condition of matters generally when leaving and when coming on duty, together with any circumstances which may have occurred during his duty, such as changes in the machines, irregularities of any description, details of the run, &c., &c.

29. Iron and steel tools should be kept clear of all machines. Metal work must not be filed in their immediate neighbourhood. A copper oil-feeder with an insulated nozzle should be employed for lubrication. Oil or water must not be spilt upon or near any machine.

30. Any instructions which may be given by the superintendent with respect to the manipulation of switches, pressure of current, or changes to be effected, are to be most scrupulously observed.

Mr Langdon.

MIDLAND RAILWAY.—ELECTRICAL DEPARTMENT.

Precautionary Instructions in relation to the Avoidance of, and for the Treatment of Sufferers by, Accidental Shock from Electrical Current.

1. If ordinary precaution is observed in handling the electrical cables, wires, or apparatus, no danger need be apprehended; but where indifference to well-known restrictions, or negligence in any form, is allowed to prevail, serious consequences may ensue, not only to the individual guilty of such negligence, but to others who may be associated with him in the discharge of his duty. Regard any apparatus with which you may have to deal as capable of affording a shock, and adopt precautions accordingly.

2. No person should be entrusted with the performance of a duty until he is perfectly competent to deal with it.

3. When it is necessary for regulation or other purposes to touch parts of such apparatus when the current is active, the individual should be careful that his entire person is insulated from the earth, and from anything, other than an insulating medium, which connects with the earth. This may be best achieved by standing on an india-rubber mat. On no account should he touch parts of the machinery, cables, or apparatus with both hands at the same time, unless his hands are protected by sound india-rubber gloves. Shoes with india-rubber soles may be worn as an extra precaution, but the wearer must never rely upon them for insulation. Dynamo-room men should keep such shoes in the dynamo room, and not wear them indiscriminately in and out of doors. The soles should be examined daily in order to ascertain that they are sound and dry.

4. The inspector and foreman of each depôt will be held individually responsible that a sufficient supply of iudia-rubber gloves are kept on hand to replace broken ones, and that one or more pairs (according to the magnitude and importance of the installation) are retained in a closet provided specially for the purpose (and marked I.R. GLOVES), so as to be readily accessible at any time.

5. Workmen are required, after using the gloves, to return

Mr. Langdon. them to the officer in charge of the dynamo or distribution room; and that officer will be held responsible that all gloves issued to workmen are returned to the closet, or otherwise accounted for by an entry in the diary.

6. Should a lamp require attention during the time the current is flowing, the lamp is to be switched out of circuit before being touched, by the switch at the base of the lamp pillar, where such is provided; otherwise by the switch on the lamp, which, it must be borne in mind, does not insulate the lamp.

7. In cases of accident where a man has become paralysed by the current and is unable to extricate himself—

(*a*) The first effort should be to switch off the current.

(*b*) Failing the power to do this, to divert the current from the sufferer.

8. The first instruction is easily effected where the accident arises within ready reach of the dynamo, distributing room, or switch.

9. The second instruction requires careful handling on the part of the rescuer. He has to bear in mind that if he touches the sufferer with his naked hands—even by his clothing if damp from rain, or the perspiration of the body—he may be placing himself in the same dangerous position as he whom he desires to rescue.

10. It is necessary he should keep himself insulated from the cable or apparatus, and from the sufferer, as well as from the earth.

11. Time is of the utmost importance, and will probably not admit of running for gloves or india-rubber mats. An instant of time may, if judiciously employed, suffice to release the sufferer, and any dry article of clothing may serve as an insulator for that short period.

12. If a piece of wire of sufficient capacity—an iron rod or any form of conductor—is at hand, the rescuer should, first, connect one end of the wire or rod to earth, and, failing a better means, divest himself of his jacket or coat and use it to insulate his hands from the other portion of the wire or bar and the cable whilst he attaches it to, or places it in contact with, the cable (or whatever

may be the source from which the current is passing into the sufferer). The attachment should be as near to that part which is in contact with the sufferer as is possible. This may divert the current to the earth and release the sufferer. Mr. Langdon.

13. Whether it does so, or not, the rescuer should, with continued caution, proceed to remove him from the position in which he is placed. He must insulate his own person by standing on a dry board, dry clothing, a bundle of dry straw, hay, &c. He must protect his hands by interposing as many thicknesses as possible of his dry clothing between them and the sufferer; and in this way endeavour to withdraw or raise and insulate the sufferer from contact with the earth. When insulated, the current will cease passing through him, and the cable, &c., may be withdrawn from contact with him. In doing so the rescuer must exercise great care that he does not place himself in a position to receive a shock.

14. Where a man is receiving a current through his person by having placed one part of his person in contact with one portion and another part in contact with another portion of a cable, &c., thereby forming of his person a portion of the circuit, the most ready means of delivering him is by short-circuiting such portion of the apparatus immediately outside the points of contact with the sufferer.

15. In cases where a man has received a serious shock, and life appears extinct, efforts similar to those employed in cases of drowning should be made to restore animation. Recent experience has shown that the D'Arsonval system has been very successful. The treatment is as follows:—

16. Lay the patient on his back in the open air. Remove his neckcloth and unfasten his collar. Open his mouth, and, taking hold of the front part of the tongue with your fingers—either bare or covered by a handkerchief—very slowly draw the tongue forward, and as gently let it go back again 16 times a minute. Be sure that the root of the tongue is acted upon and drawn forward. Continue this action until signs of re-animation are observable, which should be the case in from 10 to 20 minutes. The motion thus imparted to the tongue should be

Mr. Langdon.

regular and rhythmical in both its tractions and relaxations. If, in attempting to seize the tongue, it is found that the jaws are closed and the teeth clenched, open them by the finger if possible, or, failing this, by a wedge-shaped piece of wood, the handle of a pocket-knife, or anything of the kind that may be at hand. Keep them wedged open.

17. The object of the traction effort on the tongue is to re-inflate the lungs, and by that means re-animate the body. The rescuers' efforts should not be relaxed at the first appearance of re-animation; those efforts must be continued until there is sufficient indication that this has been secured to such an extent as to ensure regular respiration by the patient without artificial aid. The patient should then be wrapped in such clothing as can be readily got together, capable of affording warmth, and removed to the nearest hospital, infirmary, &c., or to his own residence if at hand, and placed in bed between blankets, with warm water bottles to his feet. A little brandy may be administered as soon as the patient is able to swallow, the object now being by warmth to promote the circulation of the blood.

18. Another system is as follows:—Lay the patient on his back in the open air. Remove his neckcloth and unfasten his shirt. Make a roll of clothing or anything else at hand and place it under his shoulders. The roll must be sufficiently large to so support the spine as to allow the head to fall downwards and backwards. The patient's mouth should be opened and the tongue drawn out to free the throat. The rescuer, kneeling behind the patient's head and facing him, should then grasp his elbows and draw them well over the head of the sufferer so as to bring them almost together above it, and there hold them for some two seconds. He should then carry them down to the sides and front of the chest, firmly compressing it by throwing his weight upon them. After some two seconds the action should be repeated, and continued at the rate of 16 times per minute. Under this action the extension of the arms expands the chest walls, as in inspiration or taking breath, and if the throat is clear the air will rush into the lungs. When the arms are brought down to the sides of the chest, compressing it, the air is expelled

as in expiration. The action must be regular and rhythmical, and energetically and tirelessly persisted in until the breathing of the patient has again become normal. It is possible that this may not be assured in less than an hour. Mr. Langdon.

19. If an assistant is at hand, both systems may be employed at the same time, the tongue being drawn out as the arms are raised, and allowed to go back as the arms are depressed, the combined action being as nearly as possible perfectly uniform.

20. The first opportunity should be taken advantage of, by the services of anyone within call, to obtain the presence of a medical man, who should, upon his arrival, be requested to take charge of the further treatment of the patient; the purpose of the foregoing instructions being to enable such restorative measures to be adopted as may be possible, pending the arrival of a qualified medical practitioner.

21. It is to be understood that, although there is to be no hesitation in shutting down any machinery the movement of which for the time being may endanger life, it is important that the machinery should, especially where it is employed for the lighting of railway stations, thoroughfares, buildings, &c., made use of by the public, be set in motion again as rapidly as possible. It must not be forgotten that the sudden withdrawal of light from a busy passenger or goods station, or from rooms, offices, &c., may be attended with the most serious consequences; and for this reason no time must be lost, should occasion ever arise for suspending the light, in reinstating it.

W. Langdon,
Superintendent, Telegraph Department.

Derby, *September 25th*, 1894.

The **President**: I am sure you will all agree that our very best thanks are due to Mr. Langdon for his highly practical and instructive paper, as it is full of most interesting matter, on which I hope a very interesting discussion will follow. I am sure that Mr. Langdon merits our additional thanks when you know, as I do, that he has been suffering from an attack of the universal The President.

The President. enemy, influenza, and has really dragged himself from a sick-bed to come here. I think we are much indebted to him for the pleasure he has given us of enabling us to have the paper read by himself this evening. I see the paper bristles with interesting points, and, before opening the discussion, I will just point out a few of the questions which open up, and which I hope will be dealt with adequately—the question of series *versus* parallel system of arc lighting; the proper height of the arc lamp-posts; and the very beautiful device he has shown us of the light bridge for carrying the arcs at Leeds. There is also the question of working cost. It is very astonishing to us to find that the first man who has brought a fair and unbiassed statement of the respective cost of light by gas and electricity before us, is a railway engineer; and his results are certainly very startling. I think a purely electric light man would not have dared to put forward such startling figures. I call attention also to the interest that lies in his comparative costs of steam and gas motor plant; also in his remarks on direct driving, and on the system of units to be adopted. I am sorry he has not been able to tell us more about the transmission of power for electric purposes; but it would be asking too much, when the paper is so full of other points. I believe the discussion on the paper will be a long one, and will have to be adjourned. I hope, therefore, that any gentlemen now here who have remarks to offer, but who would be unable to attend next week, will speak during the time that is still left to us this evening. I may remind gentlemen here who are not members of the Institution that we always welcome their remarks just as much as those from our own members.

Mr. Fletcher. Mr. G. E. FLETCHER: I had intended to ask Mr. Langdon some questions with regard to the paper, but I am afraid, as time is getting on, I must confine myself to simply one. The question I wish to put to him is whether in Table II. he has included any charges for depreciation or interest on capital. Table II. is the table giving the cost. Why I wish to put that question is because in some figures that I intend to give I shall give them with depreciation charges and interest on capital, and also without; it rather alters the matter.

Mr. Langdon: The figures there are simply working figures: they do not contain interest on capital and depreciation; but still I can give you that, if it is wanted, when we next meet. Mr. Langdon.

Mr. Fletcher: Mr. Langdon has told us a great deal about steam plant, and very little about gas plant. The particular installation I am going to refer to is entirely gas plant, where we use town gas costing 3s. per 1,000 cubic feet. The plant has been running since the beginning of October last. We have there three 55-B.H.P. tandem gas engines, driving three 36-unit dynamos: one of these is a stand-by, and the other two running. We are running on the three-wire system, and in setting out the work in the first case the point was gone into very thoroughly as to whether we should run on the ordinary parallel system or adopt the multiple-wire system. The multiple-wire system seemed to lend itself very readily to the Waterloo Goods Station, Liverpool. It lent itself in this way—that one side of the loading quays may be shut down while the other side is running. In the warehouse it very often happens that one side is liable to be shut down and the other side running; and when we came to weigh the whole thing up we found that, working with a fall of potential to the extreme limit of any circuit of 1½ volts—a 1½ per cent. fall—we could by using the three-wire system bring that fall, without one side being shut down, to three-fourths of 1 per cent. Then, again, we were able to run our mains singly through the warehouse—one down one side of the warehouse, another down the centre, and the third on the other side—and so branch off from these mains. Going into the whole thing, we found that the three-wire system lent itself to this warehouse much more readily than the ordinary parallel system; and in calculating out the wire, and so forth, we found it came out very much cheaper to use the three-wire. The method of distribution is really from a distributing board in the engine room itself. We have 14 outside conductors, each of these going through a recording instrument; and the middle wire, of course, going on to the middle bar with no recording instrument, because it would be absolutely useless. These are divided up: the arc circuits are kept by themselves, and the incandescent circuits by themselves; Mr. Fletcher.

Mr. Fletcher. so that we are able to get, to a certain extent, some fairly reliable figures with regard to these. Mr. Langdon tells us that his records are taken hourly. Our 14 meters are read every quarter of an hour, so that I think we can say we get a little nearer to absolute accuracy in taking our readings than he does. During the week we produce somewhere between 2,000 and 2,500 units; and we are able to do that, taking the arc and incandescent unit together, at 2·146d. per unit, including, as Mr. Langdon includes, all station charges, carbons for arc lamps, and renewal of incandescent lamps, together with any other maintenance which may crop up. The labour per unit comes out at slightly under ½d. (0·49d.). The engine room materials come out at 0·09d., the carbon renewals of lamps come out at 0·1193d., and the gas consumed per unit of current costs 1·476d. Then we come to the cost per unit for arc lighting. Mr. Langdon gives it in cost per lamp per hour. The cost per unit for arc lighting is 2·3d., and the cost of the incandescent 1·8d. The depreciation works out on this plant at 0·42d. per unit; so that we can very safely say we are producing the current at slightly over 2½d. per unit, including all charges. I may mention that we have there 720 incandescent lamps and 36 double-carbon arc lamps (32-hour lamps). We find very great economy in using double-carbon lamps, as there is really no waste of carbon. Perhaps it may interest you to know something of these gas engines, which we were practically the first people to use. The engine is built on the tandem principle, without any stuffing boxes, as one sees engines advertised sometimes; so that we have no stuffing box to keep tight, or the fear of their burning out. The front cylinder of the tandem takes the load up to two-thirds, and by means of a governor arrangement the back cylinder comes in. When they are both working, they are working on alternate revolutions; they do not work on the same cycle.

Dr. Preller. Dr. Du Riche Preller: I am sure we are all under a great obligation to Mr. Langdon for having brought before the Institution this extremely valuable and practical paper, in which I, for one, am the more interested because Leeds and Bradford (Midland) Stations, which figure conspicuously among those

described by Mr. Langdon, have been familiar to me from my early youth. Dr. Preller.

There are one or two points which I should like to mention in connection with the paper. The first refers to the use of gas plant. It appears that Mr. Langdon has not yet arrived at any definite conclusion as to the comparative cost of town gas, Dowson gas, or steam when used as generating power for supplying electric light to railway stations and depôts; and I am sure we shall all look forward with great interest to those definite results when he is able to place them before us. Speaking from my own experience, I venture to say that where, in large railway stations, town gas, or coal gas manufactured by the company itself, is available at 2s. or 2s. 3d. per 1,000 cubic feet, it is quite as economical as, if not more so than, Dowson or other poor gas, and therefore preferable, if only for the reason, as Mr. Langdon points out, that the first cost of installation of Dowson gas plant, and the space required for the same, are thereby saved. On the other hand, I should say that in smaller stations on the Midland system (because we must look to the future, and there is no doubt that electric lighting of railway stations is as yet in its infancy, and will in time be largely extended), such as Keighley, Skipton, and Settle, the use of Dowson gas plant would be of great advantage, and that petroleum engines would be even more so; seeing that comparatively small, but still important, stations of that class would not require more than 25 to 50 H.P. generating power.

The second point I wish to refer to is the distance between arc lamps in station yards, and the height of such lamps above the level of the rails. Mr. Langdon gives 90 to 100 feet as the maximum distance apart, and 20 feet as the maximum height for arc lamps of 2,000 candle-power. That is equal to about three arc lamps per acre, which is a very liberal allowance, although, of course, the density of the atmosphere has to be taken into consideration. I would ask Mr. Langdon whether he has ever tried to place the focus of 2,000-candle-power lamps at 40 feet above the rails; because it seems to me that, apart from the larger range of the light at an angle of 45°, the six-foot way

Dr. Preller. between several continuous and parallel rows of sidings would be better lighted than when the focus is at a height of only 20 feet, and the rays at 45° may be intercepted by goods trucks or carriages, being in themselves 12 to 13 feet high. I have had occasion to inspect a good many installations of that kind on the Continent—*e.g.*, in France, Germany, Italy, and Switzerland—and there the rule is, almost without exception, to place the focus of 2,000-candle-power arc lamps (12 to 16 amperes) at a height of 50 to 60 feet above the rails; the distance between the lamps being about the same as that adopted by Mr. Langdon, viz., 90 or 100 feet. Of course, especially in such confined stations as that of Bradford, situated practically in a hollow, the greater density of the atmosphere may militate against doubling or trebling the present height of the focus; but it would be interesting to know if Mr. Langdon has made any experiments in that direction.

The third point to which I would allude relates to the important question raised by Mr. Langdon, "Which is the best "system of supplying incandescent light, arc light, and motive "power to railway stations, depôts, and yards—direct or alter-"nating current?" I think there can be very little hesitation or doubt on that point. It should be, not direct, but alternating current. Then comes the question, Should it be single-phase, or two-phase, or three-phase alternating current? If the installation requires chiefly light and some power, say in the proportion of two-thirds to one-third, then it should be single-phase; if light and power are about equal, it should be two-phase; and if it is chiefly power—viz., for fixed motors, such as those for workshops, lifts, and cranes—and some light, say in the proportion of two-thirds to one-third, it should be three-phase. As a case in point, I may perhaps mention a most interesting three-phase installation of that kind, recently erected by Messrs. Siemens & Halske at Dresden, in Saxony, for supplying from one generating station light and power to the three large Dresden stations of the Saxon State Railways. The total generating power is 1,300 H.P., and there are three three-phase alternators of 330 H.P. each, the initial voltage of 120 volts being primarily transformed

up to 3,000 volts, and then again down to 100 or 200 volts, according to requirement, at the points of consumption. The 3,000-volt transmission is about 5 or 6 miles in total length. By using alternating-current in installations such as Mr. Langdon mentions, the saving in weight of the dynamos, and also in copper, would be, as we know, at least 30 to 40 per cent., as compared with direct-current machinery. **Dr. Preller.**

Mr. Langdon has also alluded to the prospective application of electric motive power for shunting and marshalling operations in railway stations. This opens up a very wide and interesting question; but, as it refers more especially to traction, and is not, therefore, perhaps strictly relevant to the discussion, I will not enter into it now.

As regards the question of cost of electric light for railway stations, I find, on working out the figures given by Mr. Langdon as to the comparative cost of gas light and electric light, that the electric light shows a saving of 20 to 50 per cent., and gives at the same time three to four times more light than gas. I think that is a highly commendable and satisfactory result; and it is, moreover, fully confirmed by the great majority of the Continental installations I have mentioned—for instance, the extensive and magnificent installations at the central railway stations of Metz and Strasburg, as well as the direct-current installations (with storage batteries) at Breslau, Wurzburg, and many others, where in some cases the saving by substituting electric light for gas is even as high as 60 per cent., owing to the fact that the cost of town gas in these places is 4s. to 5s. per 1,000 cubic feet, instead of only 2s. as at Leicester and Bradford.

In the appendix to his paper Mr. Langdon gives the very precise precautionary instructions in use on the Midland Railway for dealing with accidents such as those caused by a high-tension current passing through the body of an attendant (instruction 14, *et seq.*). The method of treatment laid down in those instructions is that known as the "Laborde" system; that is to say, the person who has received a shock of that kind is treated as if he had been drowned. As we know, the originator of this method was really M. D'Arsonval, of Paris; and as

Dr. Preller. such it was successfully applied not very long ago at St. Denis, near Paris, where an attendant had been completely paralysed by a shock of 4,500 volts, and after an hour or so was restored. In another case which I remember, and which happened at Innsbruck, in Tyrol, last year, a man was similarly struck by a shock from the 5,000-volt transmission; but in this case the D'Arsonval, or Laborde, method of treatment proved ineffectual, not improbably owing to the weaker constitution of the victim. The interesting point in connection with that accident is that it led Dr. I. Kratter, M.D., of Graz, who had treated the man, to make a series of further experiments, in order to see how animals would be affected by these high-tension shocks. He experimented on mice, guinea pigs, and dogs, and found that even when a discharge of 2,000 volts took place close to the skin of the mice and guinea pigs it did not kill them, whereas to a dog it proved immediately fatal. About the same time, two horses and several dogs were killed at Bordeaux by a shock of only 500 volts through a telephone wire which in its fall had become entangled in the conductor of the electric tramway. I mention these cases as showing that highly organised animals like horses are far more liable to be killed by even low-tension shocks than much weaker animals of the lower species by much more powerful shocks. Horses are so extensively used in station yards in shunting and in other operations, that great precautions are necessary to prevent accidents either from overhead wires falling upon them, or from accidental short-circuits in underground cables.

In conclusion, I would again emphasise the great value of Mr. Langdon's paper, and of the interesting installations which he describes, and which are worthy of the progress and enterprise ever shown by the Midland Railway.

The discussion was then adjourned.

The PRESIDENT: I have to report that the scrutineers declare the following candidates to have been duly elected :—

Members:

Albert Gay.	Jacob Stöttner.
Frederick Spencer.	

Associates:

William Gay Clarke.
William Corin.
Henry M. Darrah.
Capt. Frederick Dennett Falshaw.
Harry Stacey Howard.
Hilton Johnson.
Walter Sydney Sharwood.
Herbert Edward Soper.

Students:

Edward George Brown.
Anthony Clark MacWhirter.
John Charles Melvin Matthews.
Charles Vernon Morgan.
David Wright Niven.
Frank Berkeley Wily.

The meeting then adjourned.

The Two Hundred and Seventy-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, April 4th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on 28th March were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Students to that of Associates—

A. Alfred Beadle.	G. J. A. Fuller.
George Hawkins Cutting.	Leonard Fuller.

C. G. Paget.

Captain Sankey and Mr. Horn were appointed scrutineers of the ballot for new members.

The PRESIDENT: I have now the pleasure to announce that one of our Vice-Presidents, Sir David Salomons, has, with his customary munificence—I call it *customary*, for it is now so often repeated—increased the "Salomons Scholarship Fund" by a further donation of £500. The fund—which, you will recollect, was established by him—now reaches £2,000. I need scarcely ask you to give Sir David Salomons a hearty vote of thanks for such munificence.

The meeting signified its assent with enthusiasm.

The PRESIDENT: Before resuming the discussion on Mr. Langdon's paper, I understand from Mr. Langdon that he can add a few remarks clearing up some points on the questions of interest and depreciation.

Mr. LANGDON: On the occasion of the last meeting, reference was made to the question of interest and depreciation. I have always regarded the question of depreciation, in relation to electrical machinery, as rather a bugbear, for the reason that electrical machinery, beyond all other machinery, must be kept in the most perfect order. There is no possibility of allowing that to depreciate. Hence it is a question whether any amount at all should be allowed for depreciation. But, although everything may be kept in perfect order, it may be argued that a time will come when it will be necessary to replace existing machinery by that of an improved character, and so, perhaps, it will be right to make some allowance for this description of depreciation. That allowance, however, ought, in my opinion, to be very small. Purely for the purposes of comparison, I will take 5 per cent.* for depreciation (or for replacement of plant at some time—say within 20 years). Interest I put at 3½ per cent. That would be 8½ per cent. on the capital outlay. Based upon this, I have drawn out a statement in continuation of the comparison I made in the paper between the cost of gas and the cost of electric light at three of the sheds on the Midland Railway. The result is that, instead of effecting a saving of £1,050, we make a saving of £615. I wish to emphasise the fact that this is taking into account all charges made against the electric light. There are renewals, repairs, cleaning—every expense which is called to account—but with regard to gas we take only the cost of the gas itself. There is no provision whatever for depreciation or interest, or for repairs or cleaning. Mr. Langdon.

Mr. J. S. RAWORTH: The gentlemen who are present in this room this evening have the felicity of belonging to an Institution with a good constitution. I believe that with natural advantages in the way of health, and occasional doses of iron and quinine such as have been administered to it this evening by our good friend Sir David Salomons, the journey of life which lies before it is likely to be a long one. I am sure that we shall all feel before we have done with this paper that the two evenings which we have spent Mr. Raworth

* The author has, for convenience of reference, extended this comparison on a basis of 2½ per cent. He also desires to point out that the charges for the electric light do not include anything for rent or taxes.

Mr. Raworth.

on the Midland Railway, in a train personally conducted by Mr. Langdon, will have been amongst the pleasantest parts of that long journey. I am a constant traveller on the Midland Railway, and I look upon it as one of the greatest sources of satisfaction I have in connection with the removal of our works to Loughborough that I have to use that very pleasant method of travelling. We are treated in a most hospitable and generous manner by the Railway Company. They provide us with excellent carriages; they feed us well, and they treat us well; and I have often heard the question asked by people who are great travellers, How is it that you meet with such uniform civility from railway porters and railway guards, and everybody connected with the Midland Railway? Now, Sir, I had a little doubt in my mind as to what the reason for that uniform civility was, until I had the good fortune to meet with Mr. Langdon. It immediately occurred to me after I had come out of his office: Well, if that is a sample of the managers of the Midland Railway Company, it does not require any exercise of imagination to know how the spirit of kindness and consideration passes through the whole body of its staff, including porters and guards.

Coming to the paper, I think it is very kind of the Midland Railway Company and of Mr. Langdon to present to us the information which they have done. I have, perhaps, some experience in railway lighting. Mr. Siemens will remember—so far back, I think, as 1879—that I had the honour, and also the profit, of fitting up for the London and North Western Railway Company the harbour and goods yards of Holyhead with the best apparatus we could then command. We had five arc lamps, fed by five separate dynamos and separate cables; and, although that was a pretty considerable undertaking at the time, it has worked satisfactorily up to the present, and I believe it is still working. The next step the London and North Western Railway took was to get me to light up one of the passenger platforms in the Victoria Station, Manchester, by means of alternate-current arc lamps. That platform was a long narrow one, and we lighted it at the exceptionally low price at that time of 2½d. per lamp-hour. Now, to show you what an enormous stride has taken place

in public opinion, and in the opinion of men who are a considerable shade above the ordinary public—viz., railway managers—after that installation had run successfully for, I think, nine months—I forget the exact time—Mr. Webb told me, "The light "is very nice, but do you know it costs nine times as much as gas?" You will imagine the quality of the gas lighting. I can assure you it was not worthy of being called a light at all; but, as it cost only one-ninth of the cost of electric light, they preferred to go back to this dungeon of gas rather than indulge in extravagance. Mr. Raworth.

Now at that time Mr. Webb was personally very much impressed with the advantage of electric light for railway purposes, and he got me to give him the estimate of a scheme for lighting up Lime Street Station, at Liverpool, and the Camden Goods Station, in London. And if ever there was a case of an engineer being "hoist with his own petard," this was it, because at that very moment Sir William Siemens brought out his regenerative gas burner. Mr. Webb took a great fancy to it, and the electric light has not been heard of again in those departments until, I believe, the last month or two.

You will imagine that I, in company with almost everybody else interested in electric lighting, when we saw the title of this paper, and had an opportunity of casting our eye over the proof, ran through the pages rapidly to see what Mr. Langdon had to say upon the electric lighting of trains. Now I find that he does not say anything. I take it, therefore, that Mr. Langdon is of opinion that for the ordinary lighting of trains electricity is not yet quite practicable. I have already advanced that opinion before other gentlemen, but they do not agree with me. They say it is practicable, and that it does work well on the Brighton line, and so forth. I quite admit that it does, but you must all remember that we had in the Brighton line a most remarkable circumstance in favour of electric lighting. The locomotive superintendent, I think it was, who had charge of the matter, and he commenced the whole operation by taking up what was believed then to be a fundamental patent for the lighting of railway trains. If you want to make anything succeed in this world, by all means get

Mr. Raworth.

your customer to take out a patent; and if it won't succeed under those circumstances, you may take it from me it never will. But I know Mr. Langdon has tried the experiment of lighting the Midland trains with the electric light; and they were really very good while the experiment lasted. He gave it as good a chance as anybody could do, and if it failed I am quite sure it failed on its demerits; and I believe we shall have no opportunity in this country of seeing the electric light anything like thoroughly and properly tested in railway trains until a new line be made with no cross connections, which would thus provide an opportunity of trying electricity as the complete and only system of lighting. Under those circumstances I have not the slightest doubt that it would beat gas and oil two or three to one.

Coming to the question of the arc lamps, I see that Mr. Langdon has here got a little device which I believe I had the honour of inventing—that is, of fixing an arc lamp upon its foot; but it appears to me that he does not quite get the full advantage of the method, because I also see hooks above. I do not know whether these hooks are put in to transmit the current or to sustain the weight.

Mr. LANGDON: They are to sustain the weight. They are insulated; they are ebonite.

Mr. RAWORTH: The great advantage we find in the City of London with the method of fixing an arc lamp from its foot, is that if any repairs have to be done to the lamp—which, of course, they never have—the man can climb up the post, unhitch the lamp from its support at the foot, gently bring it down on his shoulder, and climb down the post with it on his shoulder; thus doing away with the necessity of fixing up a shear leg over the post in order to lift the lamp out at the top, as was the old-fashioned method.

I want to say just one word on Mr. Langdon's costs. If you balance up the total of the electric lighting stations which he has under his care, you will find they do not make a very great total. If they were all put together it would not be a big lighting station; and I think that Mr. Langdon must be congratulated upon the excellent results as to costs which he has

obtained from these numerous small isolated stations. I find that the highest total cost which he gives is 4d., and I think the smallest is 2·7d. Now those figures are very good. I think that, on the average, they compare very favourably with the "costs" of lighting stations situated under far more favourable circumstances. I think, therefore, that this is an example of a business well conducted by people who understand that business; for not only is Mr. Langdon a skilled electrician and a man of business, but he has a well-trained and highly organised staff under him. Mr. Raworth.

Mr. W. LEONARD: I regret I was unable to attend the last meeting to hear Mr. Langdon's paper, but, as I have been favoured with a copy, I should like to offer a few observations. Mr. Langdon, in his preliminary remarks, referred to several railways that had adopted the electric light in its early stages, but he omitted to mention the South Eastern Railway. I think it is due to Sir Myles Fenton to mention that he was one of the first to recognise the advantages of the arc system of electric lighting in its application to railway stations and the lighting of busy centres. You will probably recollect, Sir, that in 1881 you introduced a small plant at the Bricklayers' Arms Goods Station. I think it may be called our President's preliminary encampment, for it was really a small tent with a temporary lighting plant; consisting of a portable engine and about four A Gramme machines, to light four arc lamps. That, I believe, was in use for some considerable time. In 1882 the Brush Company started the lighting of the Charing Cross Station from their works, and in the same year the British Electric Light Company took up the lighting of the Cannon Street Station. The three stations have been lighted continuously from those dates. At Bricklayers' Arms we have now 26 lamps — 10 amperes — of the Brush and Brockie-Pell types, 12 outside and 14 in the sheds. The circuit extends three-quarters of a mile, and we have a Brush dynamo capable of lighting 40 or 50 lights. The lamps are switched on independently — first the inside lamps, and then the outside lamps; and in the mornings they are switched out in the reverse order. The cost last year, including all charges, was just under 1½d. per lamp- Mr. Leonard

Mr. Leonard.

hour. At Charing Cross Station, after the Brush Company had given us notice that they wished to give up the lighting, we made arrangements with the Strand Electric Light Company to supply current from their 200-volt mains. They contracted with Messrs. Crompton & Co., and I think they made a very neat and tasty finish of the lamps suspended from the roof. The lamps are supplied and maintained throughout at 5d. per unit, including the carbons; and I think, taking all things into consideration—the value of space at Charing Cross, which is one of the difficulties of our putting in an installation, and the fact that the lighting in the summer time extends only to four or five hours — the cost works out about the same. At Cannon Street, following the same example, we have made arrangements with the City of London Company to light the station, and the lamps have been running now about six months. Theirs is an alternating current transformed to 200 volts; but in each case we have made arrangements not only to divide the outside and inside lamps, but we have kept every platform distinct, so that when a platform is not occupied by a train the lights may be switched out (one of the platforms is not required after eight o'clock at night). This, of course, leads to a great economy, and I think it will be found to answer our purpose equally as well as finding our own plant; and we have the great satisfaction of knowing that the supply is continuous, and that it can be had in foggy weather at any moment, which is a great advantage, situated as both stations are close to the Thames.

I see Mr. Langdon mentions that clear globes are not a success on the Midland Railway. I may mention that our outside lamps are placed higher and further apart than Mr. Langdon's, and we find people very much prefer clear globes when that is the case.

I have no doubt that if a uniform generator could be applied for arc and incandescent lighting, there would be good scope for contractors; because one of the difficulties at railway stations is that very small plant is required, and to have duplicate machines of different types is a very great disadvantage.

Mr. E. MANVILLE: I think we owe a good deal to Mr. Langdon for a paper of such an eminently practical nature as the one which has been read before us. After all, what we seek for nowadays is more in the shape of practical information than theoretical information, and it is such a paper as this one that gives us something on which we may found our plans in the future. There are one or two points in Mr. Langdon's paper I should like to make a few observations upon. In all this extensive lighting undertaken by the Midland Railway Company the candle-power of the arc lamps is kept down to that which is given by a maximum current of 10 amperes, many of the lamps being only of 7½ amperes. I think it is apparent to most of us who have had to do with lamps of higher candle-power than these, that even a comparatively small increase in the current taken by an arc lamp is productive of great extra efficiency in the light. I may refer particularly to an installation with which I am concerned—the lighting of the streets of Portsmouth—in which, I believe, for the first time in English street lighting, lamps of 12½ amperes have been used. We feel, from our experience there, that an increase in current from 10 to 12½ amperes has been of great benefit to the general lighting of the streets. If that be so, I should think that for railway yards and open spaces of all kinds it would be very advisable to consider in the future the adoption of lamps of greater current-capacity than 10 amperes. I am inclined myself to think that lamps of 12½ or 15 amperes would be far more advantageous for lighting large open spaces such as have been referred to. Mr. Manville.

Mr. Langdon lays considerable stress upon the adoption, if possible, of plants so arranged that both arc and incandescent lighting—extended over considerable areas—may be supplied from the same plant. This is exactly what we have carried into effect in Portsmouth. The incandescent lighting is effected on the alternating-current transformer distribution system, with a low-tension network; and from the omnibus bar of the station the arc lighting is also effected by means of transformers and rectifiers. These rectifiers, I believe, were the first introduced for practical use in the kingdom, and they have proved eminently satisfactory

Mr. Manville.

to meet the conditions of series arc lighting. We certainly have gained much from the use of such an apparatus. The great economy that can be effected by using one set of plant, consisting of a number of large units, for the production of both arc and incandescent lighting is most valuable. Perhaps in the case of a railway company commencing to do electric lighting there is a tendency to creep before walking—I mean there is a tendency to put down small units of plant to light a few depôts or goods yards, before embarking on a larger scale in the same direction. This, I think, is somewhat unfortunate, since, in order to obtain high economy in the end, it is essential that a number of large units of plants should be used for the purpose. I think when one starts in a small way there is, unfortunately, a tendency not to increase the size of the units much beyond those first used. I consider that in laying out such an installation as that required by a railway company in a large town it would be very advisable to start with a sufficiently extensive scheme so as to use large units to commence with, and thus enable the extension of the plant ultimately being carried out on the most economical scale possible.

Mr. Albright

Mr. J. F. ALBRIGHT: I think it must be a source of congratulation to all the members of this Institution that we have had this paper put before us by Mr. Langdon, the more so, I think, as Mr. Langdon is an impartial witness in the cause that we most of us have at heart. Numbers of us knew many of the facts which Mr. Langdon has put before us, but we might have given them to the public many times without their having produced the same weight that they have from the mouth of Mr. Langdon. I am not, in the short time at my disposal, prepared to go into history, or else I could enumerate a number of railway stations, &c., with the lighting of which I have been connected in one way or another; but I propose simply to deal with the paper as quickly as possible. In my opinion, the great improvement in arc lamps has created, and will create, an immense future for arc lighting in connection with railway stations, goods yards, &c.; but I think there is a certain mistake in over-estimating even this, and, with no wish to detract from the case the author has so ably made out,

I think it is wrong to talk about a 10-ampere lamp as of 2,000 candle-power. It is, in my opinion, misleading. We most of us know that its actual candle-power when placed in a globe or a lantern is not much in excess of 1,000 to 1,200 candle-power. There are many other points to which I might have made allusion, on several of which I should be in agreement with the author, but the chief point I wish to touch upon is the question of the system to be adopted. Mr. Albright.

The last speaker has dealt with that to some extent, and I daresay that many of you will not be surprised to hear that I differ from him. I do differ from him. I believe that the best system to be adopted for lighting large railway stations, stores, offices, &c., is a direct-current system—whether you call it a three-wire system, or a parallel system with a middle wire, does not matter much for the sake of my argument, but for brevity I will call it a three-wire system—at either 220 or 440 volts pressure. Whatever merits alternating currents may yet be proved to have in small towns with extended areas, I believe very few impartial engineers, who are equally concerned as I am in the manufacture of alternating- or direct-current machinery, will advocate its adoption for such purposes as those which the author has put before us. I think the very fact that the author has found it advisable to give an elaborate set of rules for life-saving to some extent demonstrates what I say. I am also, on similar grounds, inclined to condemn high-tension direct-current series arc lighting, both on account of what I consider to be the needless danger of this system in such situations as we are discussing, and because all lamps under that system have practically to be of the same size. Inside warehouses, low-roofed stations, and in many places, small arc lamps may be advisable; in other places, especially outside in shunting yards, &c., I entirely agree with Mr. Manville that large arc lamps are probably amongst the best. I do not know whether Mr. Manville meant to say that 12½-ampere lamps had been first used at Portsmouth, or that Portsmouth was the first place where he had used them; but I might say at once that we have been in the habit of using 12½-, 15-, and even 20-ampere lamps in similar situations, with

Mr. Albright. efficient results. I believe that 15- or 20-ampere lamps, placed 50 to 60 feet above the ground, is the best way of lighting goods stations and that class of place.

Another reason for adopting these larger units of light is that it is always difficult, I believe, to find places in these situations for arc lamp-posts, beside which every extra lamp-post placed among rail tracks adds another chance of accident. With series arc lighting, in the event of a waggon being derailed, or any similar accident occurring in cases where a series system is used, by damaging one post 50 arc lamps would be extinguished; and I leave you to picture to yourselves the damage that would result. By the three-wire system I am advocating, four to eight lamps are the most that would be extinguished by one such accident. Returning to the system to be adopted, I repeat that I believe in the three-wire—220- or 440-volt—system; the latter, of course, for cases in which extended areas are in question. The very fact that we can now, I believe, rely on 220-volt incandescent lamps tends to make this system simplicity itself. It also enables the requirements of the author, to which Mr. Manville has also alluded—viz., to have the whole of the operations of incandescent lighting, arc lighting, and the use of motors, performed from one machine—to be easily fulfilled. At the same time, I do not agree with the author if he means that one single machine on one single engine should be adopted for such a class of work as that. Possibly he did not mean that; but I think certainly that there should be several such units besides what we may call the light-load unit. Another advantage of adopting such a system as I am advocating is that all sizes of lamps can be worked—5-, 7-, 10-, 15-, or 20-ampere lamps—according to the place in which they have to be situated. I think it is extremely probable, although it depends on the requirements of every case, that accumulators would be of very great importance and benefit in such a system as this. I believe it would generally pay to introduce them, especially with the greatly reduced first cost of accumulators now in force. My idea of how passenger stations, goods yards, store sheds, &c., should be worked is that a low-tension network should be laid throughout, and in addition to the permanent

lamps and motors there should be a number of points at which plugs could be inserted for lighting portable arc lamps, incandescent lamps, or motors, according to requirements. This is not merely an idea on my part; at the present time it is actually carried out in the East and West India Docks and in the Albert Docks, in the former of which the system has been adopted and carried out over a total distance of five miles. I am informed that the cost of the conductors in this case was most carefully considered before this system was adopted, and after being worked out it was found that on the question of the conductors alone the system that I advocate was cheaper than the high-tension system. Mr. Albright.

One word in regard to the cost of production. I believe, now that cranes, capstans, winches, pumps, travellers, hoists, &c., and all operations of that kind, can be, and are being, worked electrically, we should be able to obtain a very fair day load in such circumstances as Mr. Langdon has been drawing attention to. I think his figures, although certainly very good, will soon be able to be cut down from 25 to 50 per cent. I believe that the state of things to which I allude will be realised before long at several of the principal railway stations and depôts in this country.

Mr. R. W. Weekes: With reference to the interesting paper which Mr. Langdon has placed before us, it will be noticed from the last column of Table II. that the Midland Railway Company get their fuel very cheap; but at the same time the machinery must be well designed and looked after to keep the cost per unit so low. When we look at the labour item in the cost of production, we find it is considerably higher than in any ordinary direct-current station having an output of the same order as those given in the table. In an installation with which I am connected as consulting engineer, where we use about 65,000 units per year, the whole expenses work out to almost the average of those given by Mr. Langdon, *i.e.*, 2·96d. per unit. We use Otto gas engines, and the cost of gas works out at 1·43d. per unit. The item on which we do save is on the labour, although our charges for labour are considered heavy. I had hoped we should have had some information from Mr. Langdon in reference to the train-lighting gear with which he has experimented so largely on the Mr. Weekes.

Mr. Weekes Midland Railway Company, and of which I assisted in getting out the first designs. I have abstracted, and placed on the wall for observation, the following table of figures as to the cost of train-lighting in America. They were made up from some information given by Mr. B. Leonard in a paper read before the Association of Railway Superintendents in America, which, I believe, are reliable. He gives the cost of fitting up the train, how much candle-power is provided in each carriage, and then, last of all, how much it costs to keep the light going. In the first example

COST OF TRAIN-LIGHTING IN AMERICA.

System of Lighting.	Initial Cost per Carriage.			Initial Cost per 16-C.P. Lamp.			Total C.P. per Carriage.	Cost of Supply. Per Carriage per Day.		Cost of Supply. Per C.P.-Hour.
	£	s.	d.	£	s.	d		s.	d.	
Direct from dynamo (G. Gibb) ..	70	10	0	3	18	0	272	4	1½	0·0162
Dynamo and accumulator (Pullman Co.)	201	5	0	8	19	0	360	8	8½	0·0264
Accumulators only (Pullman Co.) ...	135	14	0	5	0	0	432	7	1¼	0·0210
" (Chesapeake and Ohio Railway)	147	0	0	15	16	0	149	3	11½	0·0244
Dynamo and accumulator (Lewis system)	104	2	0	8	14	0	192	2	0	0·0117
Pintoch gas	83	10	0	...			149	3	11½	0·0306
Oil	15	0	0	...			149	2	7	0·0206

he gives, they had a high-speed Westinghouse engine, working on an Edison dynamo at a fairly high speed without accumulators. The cost works out at £3 18s. for each lamp installed, and the upkeep cost referred to comes out at 0·016d. per candle-power-hour, as compared with oil 0·0206d., and with gas 0·0306d. But in the gas system they have a larger number of small burners, which are of 6 candle-power, and the duplex oil burners give 12 candle-power each. The Pullman Company, having only accumulators, which are withdrawn at the end of the journey to be re-charged, find their cost per candle-power-hour to be 0·021d.; and the Chesapeake and Ohio Railway Company obtain similar figures by the same system. The Lewis system is almost the same as that

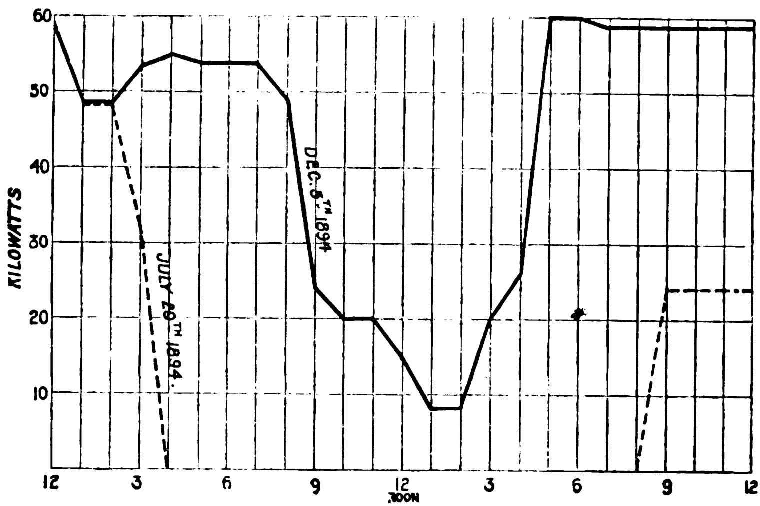
used by the Midland Company, with a dynamo in the guard's van, worked off the axle of the carriage. Mr. Leonard states that the figures with which he worked out the cost were attained after six months' working only, and so would not be quite reliable; but he makes the cost as low as 0·0117d. per candle-power-hour, which is exceedingly low. The first cost of installation amounts to only £8 14s. per lamp installed in the train. I think we should all be pleased if Mr. Langdon in his reply would give us a few figures as to how much it cost on the Midland Railway to fix the electric light on his system. Perhaps he could at the same time tell us the cost of upkeep, and also whether he obtained a higher candle-power in each compartment than they are actually getting now with the lamps which replaced the electric light, or which are used on the same build of carriages. Mr. Weekes.

Captain SANKEY: I should like to call attention to the figures in the last column of Table II., giving the cost of coal per unit. It will be observed that in every case the cost of coal per unit is less for the half-year ending December than it is for the half-year ending June. That, no doubt, is due to the fact that the half-year ending in December is darker than the one ending in June; and, therefore, the engines are better loaded—that is to say, the load-factor is larger. Mr. Langdon has given two load curves for the Derby Station—one for December 21st, and one for June 24th (Fig. 2). I have worked out what the cost of the coal ought to be for those two load curves, on the supposition that the engines were worked in the manner most suited to the varying load. I am enabled to do this because there are data at Thames Ditton for these particular engines from which the consumption can be deduced with certainty at all loads. The result comes to this—that for the December load curve the coal should be 0·23d. per unit, and for the June load curve it should be 0·84d. per unit. This is a good illustration of the great effect of the load curve on the coal consumption. The average of these figures is 0·53d., which is only very slightly less than the average of the figures given by Mr. Langdon for Derby, namely, 0·59d. This agreement is somewhat remarkable; and I think the probable reason is the care that is taken in running the engines—that is, they are stopped and Captain Sankey.

Captain Sankey.

started correctly at the right times. Anybody who has had the pleasure of seeing the station will agree with me that, although on a small scale, it is a model for all electric light stations. The way in which these calculations are made was described in some remarks I made on Mr. Crompton's paper, read before the Institution of Civil Engineers, on the cost of electric energy.

Mr. Langdon has been kind enough to favour me with two load curves for the Lawley Street electric light station,



BIRMINGHAM. LAWLEY STREET.

Birmingham; and I have calculated from them what the consumption should be, taking as the basis the only consumption figures for simple engines at my disposal, viz., those for Willans simple engines tested at Thames Ditton. The results are as follows:—For the December load curve the cost of coal per unit works out to 0·26d., and for July to 0 38d. The average is 0·32d. It will be observed that this is considerably less than at Derby; and the reason is that at Lawley Street, as I understand, the great majority of the lights are arc lights, and therefore the load-factor should be much larger at all times, and especially in the summer, as would be seen by comparing the Derby load curve in June and the Lawley Street load curve in July. The average of the figures given in Table II. for Lawley Street is 0·52d.; there

is therefore a considerable difference between the calculated consumption and the actual, no doubt due to the effect of some local conditions at Birmingham. Captain Sankey.

Mr. A. P. TROTTER: Mr. Langdon has given us a very lucid description of plant to produce a certain result. The lighting of railway stations and goods yards is one of the oldest applications of arc lighting, and will still be a very important one in the future. But the result of all this work is not putting down so much plant—that is the only result, too often, from the contractor's point of view—but the result to the company is producing a certain amount of useful illumination. On that point Mr. Langdon has not given so much information; and I have worked out from his figures what he is doing—that is to say, the actual result that has been obtained, namely, the useful illumination. It is very important that we should know what is the illumination which is found by experience to be suitable for such work, in order that future contractors may be guided by it. For goods yards there are two purposes for which the illumination is required: one is for reading the labels and for unloading the goods into the carts, and the other is for shunting. A comparatively small amount of illumination will do for shunting, as long as the light is uniform and good, but not too dazzling; but a better illumination is required for reading a label. Mr. Langdon has only given us an idea of the illumination by stating number of amperes used in the arc lamps. He does not say what the watts are. The 1,200 and 2,000 candle-power are merely nominal, of course. Even the amperes and watts are only scientific information, compared to the real practical result. It is hardly worth while talking about candle-power nowadays; it is not that that is required, but a proper illumination. The candle-power of an arc lamp varies in direction, and, therefore, in illuminating large spaces of this kind we must recognise the fact that the candle-power of an arc lamp *is* different in different directions. This is a matter of simple calculation. Mr. Trotter.

I will repeat you a diagram (Fig. 1) given in a paper which I read before this Institution in 1892. You will remember that I then showed that, if the candle-power of an arc at different angles

Mr. Trotter. with the horizon is plotted as a polar curve, this curve is practically part of a semicircle between 90° and about 60° with the vertical, and it then falls off owing to the shadow of the negative carbon. I showed in a paper which I read before the Institution of Civil Engineers in 1892 that the illumination on a horizontal plane, due to the light of an arc lamp, varies as the fourth power of the cosine of the angle inclination for all rays between 90° and 60° with the vertical. In an appendix to that paper I worked out a table of the fourth power of cosines, and from that table it is easy to calculate what the illumination must be at greater distances than the tangent of 60°. The tangent of 60° is about 1·73; this means that, if you have two lamps further apart than 3½ times their height, the illumination at a distance from either lamp more than about 1¾ its height varies as the fourth power of the cosine. The distribution of illumination may be clearly represented by means of a curve, distances being plotted horizontally, and illumination in candle-feet vertically. I have calculated many such curves, and have plotted many from actual photometer readings in the City and other places, and I have found that the actual readings agree closely with the calculations. The question is in some of these cases, Do you want a certain maximum or a certain minimum? In goods yards the minimum illumination is undoubtedly the important thing to consider. Mr. Llewlyn Preece communicated some calculations in an article to *The Electrician* in September, 1893 (vol. xxxi., p. 465), in which he calculated out the $\cos^4$ curves, and gave a diagram from which it is fairly easy to deduce the illumination for arc lamps placed at different distances apart. Mr. Llewlyn Preece is, as you know, on Mr. Langdon's staff.

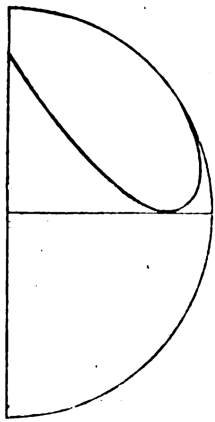


FIG. 1.

Mr. Ll. Preece assumed that the illumination falls off to nothing at the foot of the lamp-post. In practice there is a considerable amount of stray light, especially when any kind of

lantern or globe is used. The following table gives the candle- Mr. Trotter.
power at different angles of an average lamp, as deduced from many measurements, not of candle-power, but of illumination. The angles are not given in angular measure, but are expressed by their tangents—a much more convenient mode for this purpose, since the tangents represent the distances measured along the ground from the foot of the post.

Tan.	C.P.	Tan.	C.P.
0·0	216	1·4	890
0·2	348	1·6	830
0·4	520	1·8	770
0·6	785	2·0	690
0·8	970	2·5	600
0·9	1,000	3·0	507
1·0	985	4·0	415
1·2	940	5·0	308

From these candle-powers the illumination due to such a lamp, whose maximum at a little less than 45° is 1,000 C.P., has been calculated for various heights.

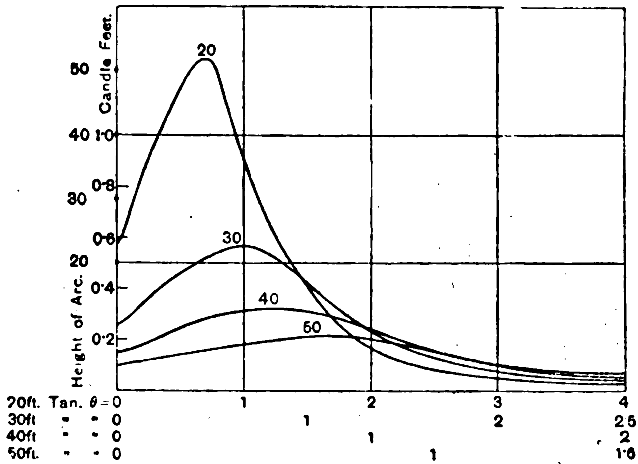


Fig. 2.

The dots on the vertical line in the diagram (Fig. 2) show lamps 20, 30, 40, and 50 feet high. The illuminations are plotted in candle-feet vertically, and distances are plotted

Mr Trotter. horizontally. With a lamp 20 feet from the ground there is a brilliant maximum at an angle whose tangent is about 0·6. The illumination is very small at an angle whose tangent is 4. If you increase the height of the lamp to 1·5—that is, 30 feet in this case—you get less than half the maximum illumination; but the minimum is considerably better. With a lamp at 40 feet high the maximum illumination is rather small compared to that of a 20-foot lamp, but it is quite enough for a goods yard; and the minimum is much better. Finally, if you go to 50 feet, beyond which rather serious structural difficulties present themselves, the maximum illumination is small, it is true, but it is spread over a wide area. There is very little variation, and a very good minimum. The illumination due to the next lamp will of course double this minimum. For ordinary street purposes 50-foot posts are out of the question—there is not enough area for them to command—but they are very useful for goods yards and other wide spaces.

I have taken the candle-power of the 6·8-ampere lamp with a maximum of 900 candle-power at 45°. You may get more, perhaps, with a laboratory measurement, but 900 is as much as you can get on an average in practice with a globe or lantern. I have taken the 10-ampere lamp at 1,500 C P. maximum. With these I have calculated the illumination which Mr. Langdon is probably producing. The first two lines of the following table give the maximum and minimum illuminations, using Mr. Langdon's data:—

C P.	Height.	Distance.	Max.	Min.
900	20	90	1·1	0·206
1,500	20	120	1·85	0·12
1,500	25	120	1·17	0·153
1,500	40	120	0·5	0·28
900	40	120	0·3	0·168

The maximum illumination is about 1·1 for the 900-C.P. lamp, and about 1·85 for the 1,500-C.P. lamp. The minimum is 0·206 candle-feet for the former, and 0·12 for the latter. I think my assumed candle-power is liberal, for about half a candle-foot was about the best that I could find in the City in 1892. If you

raise the 1,500-C.P. lamp up to 25 feet the maximum illumination falls off from 1·85 to 1·17, and the minimum is increased from 0·12 to 0·153. The maximum is almost the same as in the case of the 900-C.P. lamp at 20 feet high, but you get a considerably better minimum. But if you only want to attend to your minimum lighting, and leave the maximum to take care of itself—as I think should be done—then, assuming that you are bound to place your posts 120 feet apart, increase their height to 40 feet, and the maximum comes to 0·5, which is about the best you can get in fairly good street lighting, and is almost too good for goods yards; and the minimum will be very much increased, up to more than a quarter of a candle-foot. The theoretically best height for a light of uniform candle-power would be 42 feet 6 inches; but for an arc it should be higher, since the maximum candle-power is found at a smaller angle with the vertical. But even if you take the smaller lamp—the 900-C.P. lamp—and put that upon 40-foot posts 120 feet apart, you get 0·3 for the maximum, and you get 0·168 for the minimum; that is to say, the 900-C.P. lamp on 40-foot posts 120 feet apart will give you better minimum than the 1,500 lamp on 20-foot posts 120 feet apart. I think that is worthy of some consideration. These numbers are simply calculated with a slide rule. The third figure has no practical value: no illumination photometer works closer than 2 or 3 per cent. Mr. Trotter.

Dr. Preller mentioned that a 40-foot post was a good thing, and said they were often 50 to 60 feet high on the Continent, and 80 or 90 feet apart. You need not go as far as the Continent for such lighting. I am told that at Nine Elms the lamps are 50 feet high, but 80 to 90 yards apart. That gives a ratio of 4·8 to 5·4 as the ratio of the distance apart to the height of the lamp. That plant was put up in 1881—one lamp, one dynamo (and that, old Burgin machine)—and I believe the machines are still the same. I do not know what the candle-power is. Taking Mr. Langdon's own figures, I have calculated the ratios of distance to height. On the Midland Railway, where the height adopted is 20 feet—*i.e.*, 20 feet between the arc and the level of the rails—the ratio is 4·5 to 6. At St. Pancras the ratio is 4·3. At the far

Mr. Trotter. end of the platform, where the lights are 90 feet apart, it is 6·4. At Leicester Station, 6—that is to say, the distance between the lamps is six times their height—and at the new station at Leicester, 5·75. The minimums in the City, when I measured them in 1892, were considerably lower than might be expected. About half of the whole area of the street was below 0·1 candle-foot when the lamps were at their best; and about two-thirds of the area was below 0·1 when the lamps were at their worst, having just fed. The lighting by gas of Whitehall is just about the same: rather less than half of the whole of Whitehall is 0·1 of a candle-foot. In Great George Street you never get up to 0·1 of a candle-foot anywhere—the maximum is about 0·08, and minimum 0·005; but of course that will not do for a goods yard. In shunting it is very advisable to have fairly high lamp-posts to get rid of the dazzling. Mr. Langdon is evidently satisfied with a 20-foot lamp-post as a practical arrangement; but there are several advantages to be obtained by going to 40 feet or 50 feet, if structural difficulties do not come in the way. You can have a much bigger lamp; place them further apart, and therefore use fewer of them; you get a better minimum, and distinctly less dazzling.

It may be shown by simple geometry that, if a source of light giving uniform candle-power in all directions be placed at various heights above a horizontal plane, the illumination at a point on this plane at a fixed distance from the point below the light will be greatest when this fixed distance is $\sqrt{2}$ times the height of the source of light above the plane. This relation is discussed in my paper (*Proc. Inst. C.E.*, vol. cx., p. 82), and it is there stated that "if the height be decreased, the illumination at A will fall "off, owing to the large angle of incidence. If the height be "increased, the illumination will be diminished, owing to the "increased distance." The true result which follows from this theory is that, if the distance between the lamp-posts be fixed, *then* the height of the lamps ought to be such that the distance between them is $2\sqrt{2}$ times their height. But if, while the lamps remain at this height, they are brought closer together, of course the whole illumination will be greater. The converse is

that, if the height be fixed, the distance apart ought to be 2·828 times the height. But this is the best distance *only if* the height be fixed; for if they are brought closer together the illumination will again be greater. As a matter of practice, this relation cannot be observed in street-lighting, and a distance equal to five or six times the height of the lamp is generally chosen. With arc lamps this rule, if indeed it is worth using, must be modified, on account of the different candle-power at different angles. Mr. Trotter.

Mr. ALEXANDER SIEMENS: On the point of height of lamps and distances apart, I would remind the meeting that in 1879 we lighted the Royal Albert Docks. There the lamps have 28 amperes on the average; they are 80 feet high, and 270 yards apart. The question of what height lamps should be is an extremely simple one; it is a question of differential calculations of maximum and minimum, that will show you that the height ought to be about one-third of the distance between the lamp-posts, so as to get the maximum illumination in the centre point between two posts. Mr. Siemens.

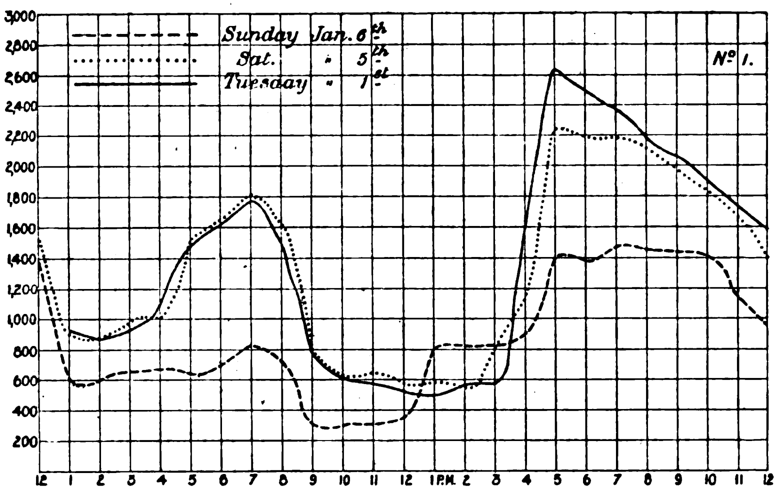
Mr. W. A. CHAMEN: I can give a few details which may be of interest to Mr. Langdon and to other members. On the question of lighting warehouses, Mr. Langdon mentions the advantages of using incandescent lamps. I would like to mention an arrangement of inverted arc lamps which was put into some of the sheds at Southampton Docks at the suggestion of Mr. Aldridge. The shed first experimented upon was one through which the whole of the American Line passengers have to pass on embarking or disembarking, and a very great portion of the cargo is also handled in the same shed. It was first lighted with incandescent lamps; the light was complained of: lamps of higher candle-power were put in; the light was still complained of, and still more lamps were put in. No satisfactory result, however, could be obtained. The roof of the shed was then whitewashed throughout, and 12 10-ampere inverted arc lamps were put up in place of the whole of the previous incandescent arrangement. The result was a considerable economy in current, the arc lamps using, if I recollect rightly, about half the amount of energy taken by the incandescents; the Mr. Chamen.

Mr. Chamen. lighting, on the other hand, was everything that could be desired: the smallest writing on any of the labels on packing cases could be read with ease in any part of the shed. The shed measures approximately 350 feet long by 120 feet wide, and the height from floor to principals is about 8 feet on the side where the trains come in, and I believe about 12 feet on the other side. It has since been decided to light some of the other sheds in the same way. On the question as to the system which should be employed in future to meet the conditions of general railway lighting, I certainly think that for such cases as Mr. Langdon has dealt with, a three-wire system at either 220 or 440 volts, or possibly even higher still, will be found to answer every requirement. The Royal Albert Docks installation has been mentioned, and I am sorry that Mr. Stanley Bright is not here this evening, as I am sure the information he could have given us would have been very interesting indeed; but I might say that, as Mr. Albright has already mentioned, the total distance which will ultimately be covered from this central station will be five miles—that is to say, two and a half miles to the end of the Royal Albert Dock, and another two or two and a half miles in the Victoria Dock in the other direction. On this system the arc lamps are worked eight in series. They are 15-ampere lamps, and they are fitted on the 80 foot poles which Mr. Siemens has referred to. The lamps in this installation are not high-voltage lamps; that is to say, they are 110-volt lamps coupled two in series. At the time when the installation was altered, 220- to 230-volt lamps were not in a fit state to use generally; but it is a fact that the Bradford Corporation have recently issued a notice to the effect that all installations which are intended to be coupled to their mains are to be wired in future for 230-volt lamps. I do not think I need take up your time by referring to the facilities which this system gives. I do hope Mr. Langdon and others who have talked about the matter will really see that the one type of dynamo does everything which is required. There is nothing else necessary at all. The plant in the Royal Albert Docks consists of three engines with dynamos at each end, each dynamo giving 220 volts.

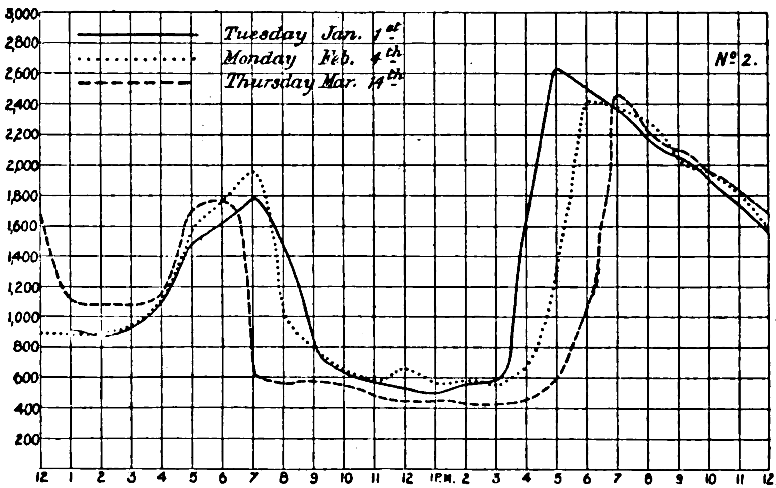
There is one branch of railway work in which high-pressure alternators and transformers, and possibly rectifiers also, if any arc lamps are required, will be found the most economical arrangement: I refer to the lighting of a series of suburban railway stations from one generating station. I do not know, however, that any railway company has undertaken this at present. Mr. Langdon refers to the difficulty which most people complain of with regard to balancing the three-wire system. The difficulty is a real one; but in one large installation I know of, by carrying the three-wire system into every place, and, where more than five lamps are in one room, into every room also, it is possible to keep the balance within about 20 amperes in a total of 1,100 or 1,200 on each side. I do not, however, suggest that it is possible to obtain an absolutely perfect balance, but mention this fact as I believe it to be about the best example of good balancing which can be shown. Some central station engineers report that for days together they do not have to run their balancing sets, but then things suddenly alter, and without any notice they find themselves several hundreds of amperes out of balance. This, however, does not occur in the installation of which I am speaking. Mr. Langdon has given us the load diagram at Derby; and, as the installation of the Great Eastern Railway has been referred to by him, I thought perhaps it would be interesting to the meeting to see the load diagrams of that central station, which supplies Liverpool Street, Bishopsgate, and Bethnal Green Stations, as well as all the general offices, hotel, &c. I am able, by the kind permission of Mr. Wilson, to give you the two diagrams which are now shown. Mr. Chamen

Referring to Diagram No. I., the ordinary week-day load is shown by the full line, the Saturday load is shown by the dotted line, and the Sunday load by the broken line. It will be noticed that on Saturday at five o'clock the load does not reach its usual total of 2,600, but only comes to 2,200; showing that the general offices are not consuming the usual amount of current. On Sunday a very marked difference is observable, both in the morning and the evening, owing to general offices and goods depôt, &c., being practically closed. The sudden rise between

Mr. Chamen. twelve and one o'clock in the middle of the day, I believe, was caused by cloudy and dark weather coming on; otherwise the load would



not have risen on Sunday before about three o'clock, as on other days. The other diagram (No. II.) shows the alteration in the



load diagram from time to time as the days get longer. The full line commences January 1st; the dotted line, February 4th; and the broken line, March 14th. I am unable to give a summer diagram, as only a small part of the lights were connected during

last summer, and it would serve no useful purpose to give the load Mr. Chamen diagram at that time. The day load, however, will never be as small as in the Derby central station, owing to the great number of underground and dark places which require light all day. Perhaps, as this installation has been referred to by Mr. Langdon, a brief description of the plant would be of interest. The plant consists of four 200-H.P. sets and three 100-H.P. sets. It is, of course, at present much in excess of what is really required to drive the load; but the generating station has been completed before it has been possible to get all the lighting connected, and there still remains a considerable quantity of lighting to be done, part of which, however, is to be in buildings not yet erected. This plant is of a somewhat unusual type in this country, the 200-H.P. sets having armatures 6 feet in diameter and 10 poles, and running at 120 revolutions per minute, direct coupled to engines built by Messrs. Davey, Paxman, & Co. The 100-H.P. sets are somewhat similar, but not of such large diameter; they are made with four poles, and run at about 150 revolutions a minute. Steam is supplied by five Davey Paxman Economic boilers of 200 H.P. each.

On the question of comparative cost of gas and electric lighting, the following two cases are interesting:—

1. *Crystal Palace.*—Plant consists of a Galloway engine capable of indicating somewhere about 130 H.P., and a Lancashire boiler. Engine drives three 550-volt compound Crompton dynamos by means of countershafting and belting. Each dynamo supplies 33 10-ampere arc lamps, arranged in three circuits of 11 each. Lamps are suspended by means of hoisting gear from the 60-foot gallery, and are generally kept at a height of about 17 feet 6 inches from the ground. They are spaced about 56 feet apart in two straight lines. Hours of burning vary considerably, the longest run being seven hours, and the shortest 30 minutes; so that in summer more coal is wasted in getting up steam than is used in running the lights. The results of the year's working from October, 1893, to October, 1894, which have been very kindly given to me by Mr. Carr, the chief engineer of the Crystal Palace, are as follows:—Total units generated, 45,584; total lamp-hours, 82,880;

Mr. Chamen. cost of coal, 19s. 6d. per ton; total cost per unit generated, including coal, oil, waste, and petty stores, carbons, and all wages, 3·2d.; total cost per lamp-hour, 1·77d. Taking into account depreciation—or, rather, redemption—reckoned on the original cost of the whole plant, some items at 10 per cent. and some at 5 per cent., but most at 10 per cent., the total cost per unit comes to 5·21d., and the total cost per lamp-hour 2·87d. Of course the efficiency of this plant is not so high as would be obtained with modern-type direct-driven sets. The I.H.P. required to drive the 99 lamps is slightly under 100; whereas, when running the engine, countershafting, belts, and dynamos light (that is to say, with the brushes lifted), indicator diagrams show 22·5 H.P.

Comparison with gas: When the boiler is laid off for washing out, the old arrangement of gas is resorted to, the result being a very miserable light, and a consumption of 13,506 cubic feet per hour, which at 2s. 6d., and 2s. for wages of gas-lighters, equals a total of £1 16s., whereas the cost of the 66 arc lamps which light the same space as that lighted by the gas, at 1·77d. per lamp, equals 9s. 7d. The remaining 33 arc lamps are used to light spaces which are left in total darkness when the gas is used, and therefore are not counted in arriving at the cost. It is interesting to notice that the gas jets are fixed 60 feet high, and that they cannot be used at a lower elevation on account of the damage done to the plants.

2. *Agricultural Hall.*—Plant here consists of two 55-H.P. Stockport gas engines and two 110-volt × 310-ampere Crompton dynamos, one engine and dynamo being kept as a spare. Forty-six 10-ampere arc lamps, arranged two in series, are supplied from these dynamos, and the actual gas consumed equals 1,050 cubic feet per hour, which at 2s. 10d. per 1,000 equals 3s. Taking the cost of carbons to be the same as at the Crystal Palace—viz., 0·26d. per hour, or 1s. for the 46 lamps—oil, waste, and wages—which for want of exact information I have taken to be the same as in the case of the Crystal Palace—at 1·05d. per lamp-hour, the total cost comes out at 8s. 3d. per hour. I am informed on good authority that the gas which was previously consumed for fully lighting the same area comes to a total of 8,848 cubic feet, which at 2s. 10d. per 1,000 equals 25s.

Mr. A. H. PREECE: Mr. Langdon has referred in his paper to the installation of the Great Northern Railway Company at Holloway, and I therefore think that a brief description of what we have done there might be interesting to the members of this Institution. We have installed there altogether 1,370 I.H.P., and the plant is capable of lighting 500 10-ampere arc lamps and 20,000 8-candle-power glow lamps. We had to deal with a problem of lighting a long narrow length of line, comprising goods yards, carriage sidings, main stations, and suburban stations, the total length being some seven miles. It was utterly impossible to do anything but use the alternating-current system. Mr. Preece.

The generating station is situated in the Holloway goods yard, which lies about two miles from King's Cross. Light will be provided at King's Cross passenger stations (main line and suburban), King's Cross goods sheds and yard, Holloway, Finsbury Park, Harringay, and Hornsey stations, goods yards, and sidings.

The generating station consists of a large boiler house and chimney, a spacious engine and dynamo house, and the necessary workshops and offices.

There are five Lancashire boilers, each 28 feet long and 8 feet diameter. These are worked by means of mechanical stokers of the Bennis type. Coal is shot straight from the waggons, by means of shoots, into the stoker hoppers. Thus the coal is never touched by hand. A Green's economiser of 288 tubes is also provided for heating the feed water.

The engine and dynamo plant consists of—

Glow Lighting Plant.—Three 200-H.P. Fowler slow-speed horizontal engines, each driving one 100-kilowatt Mordey alternator; two 100-H.P. engines of the same type, each driving one 50-kilowatt Mordey alternator; two 30-H.P. Browett-Lindley vertical engines, each driving a Brush continuous-current dynamo for exciting the above alternators. The current is generated at a pressure of 2,000 volts, and transformed at sub-stations to 110 volts.

Arc Lighting Plant.—This consists of five 100-H.P. Fowler horizontal engines, each driving two 55-light Brush arc light machines. The current generated is taken to two switch-boards—

Mr. Preece. one for the glow lighting plant, and the other for the arc lighting plant. The whole of the circuits, both arc and glow, are arranged in duplicate: that is to say, into each sub-station for the glow lighting two branch feeders are carried, each being connected to one of the four main feeders; while every alternate arc lamp in any yard or building is lighted by one of two circuits which run everywhere side by side.

The whole of the cables are of Messrs. Siemens's manufacture, the glow light being their armoured lead covered into concentric cables, and the arc light being their wire armoured rubber cables.

At present we have 300 arc lamps installed out of the 500, and 7,000 glow lamps. The principal point which I think will interest members is our costs. We have had a fair load on since July last. Our generating costs come out at an average of 2·67d. per unit, exclusive of carbons and cleaning arcs. The total comes to 3·37d. The interest and depreciation have to be added to that, which come to about 1d. a unit. From December to March—which is, of course, a time of heavy load—our costs have gone down to under 3d. per unit; and we hope, as our load is continually increasing, to keep our average down to this throughout the year. Coal is about 0·9d. per unit, and the wages, which is another big item, is 0·97d. per unit. As regards the wages, we are rather in a difficulty there; because the station is under two heads, the locomotive department taking the engines and the boilers, and the telegraph department taking the electrical part; and the result is that there are two complete staffs at the station, which is not quite conducive to economical working. As regards the height of the lamp-posts and the distances of the lamp-posts apart, we do not, in putting light in the goods yard, go into figures like Mr. Trotter has entered into. In goods yards they want lights at certain points where the lines branch, and we have to put an arc lamp there; so that all over the yard the lamps are scattered about just where the points come. The brilliant light is therefore placed where the crossing over and shunting takes place. In the goods sheds themselves of course they want as much light as we can give them for the money. In the King's Cross goods sheds the lamps are placed about 16 feet high and 40 to 50 feet

apart; in the goods yards they are placed on 20-foot posts and about 80 to 100 feet apart. Mr. Preece.

Mr. LANGDON: Might I ask you the amount which you allow for depreciation?

Mr. A. H. PREECE: The depreciation is nearly ¼d. per unit We calculate it rather in a complicated way.

Mr. LANGDON: What I meant was how much per cent.—5, or 7, or 20?

Mr. PREECE: We calculate it at 5 per cent. per annum on the cables, trunkings, and cost of laying; 2 per cent. on the buildings; 2½ per cent. on the total capital expenditure for the works plant – that means the running machinery; and 5 per cent. per annum on the proportion of works plant actually used during the week. It is a little complicated, but I think it works out in a very fair way. The sum upon which the last item of 5 per cent. is based is calculated by taking the proportion of plant-hours that the stations could possibly run to the number of plant-hours which they did actually run, and that fraction is taken of the whole capital cost of that plant. The total depreciation therefore amounts to between 4 and 5 per cent.

Mr. MORDEY: I am afraid my nerves are in rather a shattered state to-night. I have nearly seen a ghost – the ghost of the dead low-tension argument—rising in two or three parts of this hall. Mr. Langdon asks in the latter part of his paper what is going to be the future of the distribution of electricity for the general purposes of railway working; and I imagine that Mr. Arthur Preece has just given us the information we expected to receive—and, indeed, anticipated by the author—for Mr. Preece says that it was found to be impossible to carry out a broad scheme of lighting and distribution which they had in view at King's Cross by any other than a high-tension system. I suppose that, as Bismarck said the other day, we must either fight or fossilise; so, at the risk of re-opening a question that most of us thought was closed—although it is not possible to go into the question at any length now—I would ask those who are interested in railway work to consider whether future progress will not be in the direction Mr. Mordey.

Mr. Mordey. mentioned by Mr. Preece, where not only the offices and sidings, and all the various parts of the railway system about the station, are supplied by one generating station, but also all the stations away down the line—at any rate, in all suburban or busy railway districts. It is not possible, I think, by any two- or three-wire system to carry out work on a scale of that sort in any satisfactory way. I should like, if it had been earlier in the evening, to have taken up this question, not as an advocate of alternate currents, but as an advocate of electrical engineering in the broadest possible sense; but it is not possible to do that now. I can only thank the author for the very practical character of his paper, and particularly for the costs which he has given us. The arc lighting which Mr. Langdon has carried out has nearly all been done on the series high-tension system, and there is no doubt whatever that that system has very many advantages. It does not require any support from me; there is no other system so good where you have to deal with arc lighting, and arc lighting alone. It is certainly the best; it gives you direct current for your arcs. And I am an advocate of direct current for arcs where you do not want a large amount of other work that can be done better in other ways. If the arc lighting is a comparatively small portion of a large scheme, I think that the arc lighting should be done also by alternating currents or by rectified alternating currents, if you do not mind a certain added cost and complication. Mr. Ferranti is engaged, as we know, in introducing rectifiers for this work. I am sure that I wish him success, not only for his own sake, but because it enables us to combine the advantages of the two systems, and gives us what some of us think is often an advantage in arcs—a downward light. I am getting a little shaky on this point, because I have always been taught that the downward light was an advantage, and that that was one of the reasons why alternating-current arc lamps could not be used. Another argument often advanced was that alternate-current arc lamps did not give any light at all: that idea, I think, has been exploded, and people know that they give as good a light, under proper conditions, as direct-current arcs. But now my principles are getting upset,

because the direct current advocates are beginning to think that the right thing to do with the direct-current lamp is to put it upside down, so that it throws all its light up. That is being put forward as the coming method of direct-current lighting. Now the alternate current has the advantage that it gives us the happy mean—it sends half of the light each way. But on a really large and comprehensive system of distribution for general purposes, where you have to supply not only the neighbourhood of the station, but all parts of the railway system,—where you want low tension for some parts, and high tension for others,—where you have to do motor work as well as lighting,—where you want arc lamp circuits with various currents and with various numbers of lamps,—where you want to be able to turn out lights at will on these different circuits, not using plant of two or three sorts, each available only for its own work, but where all parts of the plant are equally available for all kinds of the work,—and, in short, where you want the most generally useful and flexible method applicable to all classes of work, even if not the most perfect for every one of those classes,—then I venture to say that in its largest aspect such work would be most satisfactorily carried out by alternate currents. Mr. Morley.

Mr. JAMES N. SHOOLBRED: The paper, in describing the various and important works therein cited, at large and busy railway centres, presents many points worthy of comment, and upon which further information might be asked. But, seeing the lateness of the hour, I will confine my remarks to one point. While following the various descriptions given in the paper of the lighting arrangements at several railway stations, both for passengers and for goods, I cannot help being struck by the almost entire absence of precautionary arrangements, to guard against extinctions in the lighting, or interruptions in the supply of electrical energy. Mr. Shoolbred.

I have also felt considerable surprise, that no notice whatever has been taken by any of the speakers in the discussion of this point, which, to me at least, presents itself as a blot, or imperfection, upon the very fine series of lighting undertakings which have been designed and carried out by Mr. Langdon; inasmuch

Mr. Shoolbred. as in large, and at times very crowded passenger stations, of the kind described by Mr. Langdon, it is a very serious thing, and one indeed fraught with much danger both to life and to property, to have the station, and its adjoining waiting-rooms and other adjuncts, suddenly plunged into complete darkness for some five minutes or more, as I myself have seen; and thus to leave a crowd of people in a state of bewilderment, and a part of them almost in a condition of terror. This matter is one of very great importance, and likewise one to which, in my opinion, sufficient attention has not been given, not merely in the lighting of railway stations and goods yards, but also in the supply of electrical energy to towns.

It is just as essential in electricity town supplies to avoid extinctions and interruptions as it is with a corresponding gas supply. As, therefore, a town supply of the latter would certainly *not* be tolerated nowadays without a gasometer to fall back upon; why then should the electricity supply be permitted without its corresponding reserve in the storage battery?

Mr. Dowson. Mr. J. Emerson Dowson [*communicated*]: Apart from the general interest of Mr. Langdon's useful paper, I feel sure that many members of the Institution will be grateful to him and to the Midland Railway Company for incurring the trouble and expense of a thoroughly practical trial of gas power for electric lighting at the new joint station of the Midland and London and North Western Railways. Partisans, like myself, believe that gas power is destined to play an important part in the electric lighting of the future; but what we all want is the independent and unbiassed opinion of some one who has the facilities and the ability to give it a thorough trial. Our thanks are therefore due to Mr. Langdon and to his staff for what is now being done at Leicester.

Regarding the paper itself, may I suggest that a word or two be added to the first part of the second paragraph on page 300, to make the author's meaning more clear? "Ordinary gas plant" might mean an ordinary plant for making coal gas; but I presume this paragraph refers to gas engines without a gas plant of any kind, and worked with gas from a town supply.

I think the author is wise not to commit himself to a definite statement as to the result of working with gas power until he has

tried it longer. At the same time, he tells us that we may accept provisionally that the arc lamps cost 1·2d. per lamp-hour, and the glow lights 2·3d. per unit. I would therefore ask him to be good enough to tell us approximately what proportion of these amounts may be taken to represent the cost of the gas power only, so that a more direct comparison may be made with steam power. I believe I am right in saying that for special reasons there are 19 men employed at this station—two shifts of six men each, and one of seven. There are, however, only two men employed for the gas plant—one for the day and one for the night shift—so that the cost of wages for the gas power is small compared with the total cost of wages included in the above figures. Mr. Dowson.

In conclusion, I would suggest that some reference be made to the two independent engines and dynamos which serve the glow lamps, and which are connected together in parallel. I believe they run very steadily together, even under a light load; and, so far as I am aware, this is the first time this has been accomplished with gas engines.

Mr. F. W. Webb (London and North Western Railway) [*communicated*]: I have read with great interest Mr. Langdon's paper on the Midland Railway electric lighting, and upon Table II. the following points arise, upon which I think it would be both interesting and useful to the members of the Institution to have Mr. Langdon's reply. Mr. Webb.

1. The Table No. II. is said to include "maintenance" and "renewal."

(*a*) Are the repairs taken at the actual expenditure within the periods, although the machinery is new? or are percentages applied to original costs which can be held to cover the maximum that will be expended in repairs per period over an average of years? If the latter, what percentage are applied—

(1) For boilers,
(2) „ engines,
(3) „ dynamos,
(4) „ arc lamps,
(5) „ cables, wiring, &c.,
(6) „ buildings?

Mr. Webb. (*b*) As renewals cannot yet have taken place, to any appreciable extent at any rate, I presume percentages on original costs have been adopted. What are they for—

(1) Boilers,
(2) Engines,
(3) Dynamos,
(4) Arc lamps,
(5) Cables, wiring, &c.,
(6) Buildings?

} Otherwise, what is the life taken in each case?

In adopting these percentages for ultimate "renewals," is allowance made for the interest that would be accruing on the money set aside? In other words, is the item taken in the nature of a sinking fund, or the percentages taken in the gross? It makes an appreciable difference.

2. Does the cost of coal as introduced into the statement include carriage from colliery to point of user, and waggon hire, or simply cost of coal at pit plus foreign toll? And is any charge introduced for the carriage of working stores or repaired materials?

3. Interest on capital outlay on land, buildings, and plant, not included; neither, rates paid on local assessment. Are not both serious omissions in compiling a statement for comparing the cost of generating current with the prices paid to a public company?

4. Cost of carbons and of carboning ought not to be included in the cost of current, where it is desirable to compare results with the outside price of current. Table would have been more interesting and useful if cost of generating had been shown distinct from that of carboning, &c., and number and power of arc lamps shown. Can cost of current still be shown separately?

5. In comparing with gas, interest on capital outlay and rates, &c., should be included in the cost of the electric light, as it is in the purchase price of gas, whatever may be the case at Somers Town, where the Midland manufacture themselves, and charge out at 2s. per 1,000 cubic feet. What does the latter rate really cover?

6. Is any percentage, and, if so, what, included in the item for wages or materials, or both, to cover superintendence, office or other incidental expenses? Mr. Webb.

The following is a comparison of the Midland Somers Town with the London and North Western Broad Street figures for the December, 1894, half-year, so far as Crewe knows on the same basis, with, in the case of the London and North Western, the addition, but shown separately, of interest, depreciation, general charges, and carriage:—

Installation.	Total Units.	Total Cost.	Cost.							
			Per Unit, including all Charges.	Per Arc-Lamp-Hour.	Per Unit for Incandescent Lighting.	Per Unit.				
						For Labour.	For Material.	For Repairs.	For Coal.	Total.
		£ s. d.	d.	d.	d.	d.	d.	d.	d.	d.
Mid. Ry.—Somers Town, Dec., 1894, half-year	187,326	2,506 6 4	3·20	1·60	...	1·09	1·03	0·27	0·82	3·21
L. & N. W.—Broad St., Dec., 1894, half-year	147,480	1,379 18 1	1·73	...	...	0·66	0·45	0·08	0·54	1·73
Do. interest on capital	...	...	...	...	...	...	...	...	...	0·26
Do. depreciation ..	...	...	...	...	...	...	...	...	...	0·24
Do. rent	...	...	...	...	...	...	...	...	...	0·04
Do. rates	...	...	...	...	...	...	...	...	...	0·06
Do. general charges...	...	...	...	...	...	...	...	...	...	0·13
Do. carriage	...	...	...	...	...	...	...	...	...	0·39
L. & N. W. R grand total—*i.e.*, interest, depreciation, rent, rates, working, and repairs	...	...	...	...	...	...	...	...	...	2·85

In the above comparison, in the case of repairs, the London and North Western figures represent actual payments, not a percentage on the value of the plant, believing that it is the same in the case of the Midland figures; but in arriving at the cost of current for comparison with the purchase price, a percentage which

Mr. Webb. would cover repairs over the average of a period of years was taken, and brought the total figure for current up to 2·29d.

Add for carbons and carboning, &c. ... ... 0·73d.

—— 3·02d.

Mr. Esson. Mr. W. B. Esson [*communicated May 2nd*]: I have read Mr. Langdon's paper with interest, but that his figures can be easily beaten is shown by the figures below, which give the cost of running the installation at Stoke-on-Trent Station, the head-quarters of the North Staffordshire Railway:—

Cost Per Unit in Pence.						
Coal.	Oil and Sundries.	Labour and Supervision.	Interest and Depreciation.	Carbons and New Lamps.	Repairs.	Total.
0·302	0·098	0·904	0·290	0·243	0·061	1·898

Average output for three months—2,785 units per week.

Here the total cost for the first three months of the present year comes well under 2d. per unit, including carbons for the arc lamps; nor will this be very much increased for the summer months. The installation consists at present of 24 arc lamps for lighting the platforms and yard, and the equivalent, in different candle powers, of 1,100 8-candle-power incandescent lamps, lighting the waiting rooms, passages, &c. The output varies between 2,500 and 3,000 units per week, the average for three months being 2,785 units per week. The boiler used is of the "Economic" class; the engines (two in number) are compound, by Bumsted & Chandler; and the direct-coupled dynamos, of the over-type pattern, are of my own design. The figures have been kindly supplied by Mr. Neale, the company's telegraph engineer, who is responsible for the general arrangement of the installation, and to whose specification the work was carried out by the Gülcher Company, with which I was then connected. I believe the above results have not been beaten, and so satisfied are the directors that orders have been recently placed for doubling the plant.

The PRESIDENT: The subject of Mr. Langdon's paper is one of great interest to me, and I wish I had time to speak at length on it. I commenced my electrical engineering career by doing this kind of work, *i.e.*, lighting railway stations. I supplied arc lamps to St. Enoch's Station, Glasgow, in the very early days, when very few of those in this room had commenced thinking of the subject of electric lighting; as I think that Mr. Siemens and myself were the very first men engaged on this work. When I tell you that St. Enoch's, King's Cross, Bricklayers' Arms, and Nine Elms Stations were all lighted by arcs by 1881, you will see that railway lighting has now reached a very respectable old age. There is one point which has not been touched upon, and that is, the enormous debt which railway men owe to the electric light. I do not think the great saving of life that has followed on the use of electric light in goods yards has ever been sufficiently made known. I have it on the authority of Sir Myles Fenton that at one yard only—viz., Bricklayers' Arms—the average saving of life amounted to more than two men per year; and the same is probably the case at Nine Elms and in all these yards. I think that when once that was known, the railway companies were somewhat to blame that they did not take up electric lighting more quickly than they have done; even now, after 14 years, some of the large railway companies have hardly taken it up at all, and yet the fact above mentioned, which was known to Sir Myles Fenton, ought to have been known to them as early as 1881. I am not going to be tempted, even by Mr. Mordey, to go into the old subject of alternating currents. Mr. Mordey could not choose a worse position for the employment of high-pressure alternating distribution than a railway goods yard. In such places we must always reckon on accidents occurring to the posts by the detachment of waggons or by collisions, and at such times when the conductors are liable to be exposed. I think it would be an absolutely sinful thing to use dangerous currents in such situations, and to add to the horrors and dangers of a railway accident by the extinction of a large number of the lights at the same time. The President.

Mr. Trotter has really communicated a most valuable part of this discussion, as, after all, one of the most important things for

The President. all of us to know is what is the proper height and power of the lamps. I agree with the practice of Mr. Siemens, who states that his firm in very early days hit on what really has been the best practice, and which coincides with the practice of my own firm. Mr. Manville ought to know that for such lighting 15- and 13-ampere lamps have been the rule, and that 10-ampere lamps are the exception. Mr. Langdon only used 10-ampere and smaller lamps because the plant came over from America equipped for that size. It was an American practice to use 10-ampere lamps 15 feet high; it was the English practice to use 15- to 20-ampere lamps 40 to 60 feet high. I think there is nothing easier nowadays than to plan an electric light installation for a railway company. As Mr. Albright and Mr. Chamen have said, it is easily carried out, even when the distribution extends over very great distances, on the simplest of direct systems; and when you get to 400 or 500 volts, which is probably the preferable system of all for transmission of power and lighting as well, you have got a most happy combination. You are able to use machines all in parallel, as they are in central electric lighting stations, and you need not introduce any complications or novelties. You have the simplest form of plant, and the simplest form of network of mains, which can be so interconnected that severance at any point need hardly affect the general lighting, and which, as they carry currents at a non-dangerous voltage, can be handled and repaired at any time without the necessity of switching off the pressure. Railway engineers will find that these advantages greatly outweigh the small reductions of cost which follow on the use of higher voltages.

I should have greatly liked to have discussed the costs question, but it has been ably dealt with by others. The author is to be congratulated on having secured for his company such very excellent results, and we are to be congratulated on the very valuable addition to our stock of knowledge which his paper and the discussion on it has afforded to us.

Mr. Langdon. Mr. LANGDON (in reply): The discussion has taken so wide a range that I feel it will not at this hour be possible for me to give a full reply to all that has been said. The paper which I have

had the pleasure of laying before you is one of a very unpretentious character. It is mainly a statement of what has been done by one of the larger railways in the kingdom in the application of the electric light to its traffic purposes, and the results which have been achieved thereby. I think, gentlemen, I shall have you with me when I say that these results have shown how far-seeing and how judicious has been the action of the Midland Board in calling to their aid the electric light for this purpose. The object of the paper has been to lay before, not only the members of this Institution, but others who may feel any interest in the question, what has been done, and also to seek the observations and advice of gentlemen who are advocates of various systems, that they may afford us the benefit of their opinion with respect to future advancements. The time no doubt is approaching when railway companies will require to make use of electricity for other purposes than lighting, and for lighting also to a very much greater extent than has been the case hitherto. We have, up to the present, laid down installations—as has been remarked—small installations, hardly worth considering. In the near future, in all probability, if directors can see their way to admit of the outlay, it will, at large commercial centres, be desirable for economical working to adopt central stations, and to employ those central stations not only for lighting purposes, but for other purposes, such as traction or power. The power thus generated at these central points, whether employed for lighting or other purposes, may, and probably will, have to be carried to a considerable distance from those central points—five, six, or seven miles, as the case may be. The installations that have been laid down at Liverpool Street and Holloway may be taken as a type of that which we shall probably find desirable to establish at other places. Hence, gentlemen, the desire which I felt, in submitting this paper to your notice, of directing attention to these points. I cannot express sufficiently my thanks to those gentlemen who have contributed information upon the subject. I make no doubt that this paper will be read by other than members of this Institution; and not only are my thanks thus due to these gentlemen, but the thanks of all those who, interested in the

Mr. Langdon.

Mr. Langdon. subject, may be induced to read the paper. I had endeavoured in the opening of the discussion to classify the remarks of the several speakers under different heads, but it has been so rapid that I am afraid I have not been very successful.

I will, however, go carefully through the whole of the notes which I have, and supplement these hurried remarks by such other observations as I am able, if I may do so, in writing. I should like, however, at the present moment to express my thanks to Mr. Raworth for the very kind and complimentary manner in which he has referred to the Midland Railway. We are always very anxious indeed to make our clients as comfortable as possible, and I hope we may be favoured with the support of all those in this room. We shall be glad at any time to offer facilities for the inspection of any of our installations.

I cannot sufficiently express my thanks to Mr. Trotter for the admirable exposition he has afforded us in relation to the elevation and the arrangement of arc lights. I fancy, however, that the lighting of railway yards is somewhat different to that of distributing an equality of light over large areas. We want, as Mr. Arthur Preece mentioned just now, to place our lights in such a position that the men on the ground may have the full benefit of it at that point, and not an equal amount of light spread over a large area. I think for that purpose the height we have chosen is the best which we can obtain.

I scarely feel myself competent at the present moment to offer an opinion upon those different systems which have been advocated. All I can say is that not only myself, but others also, will, I feel, read and study these observations, with the greatest desire to make the best use of them in the interest of those whom we represent. I feel that I am very much indebted to you, gentlemen, for the kindly manner in which you have received the paper, and for having added so largely to its interest by the manner in which you have entered upon its discussion.

[*Communicated April* 30*th.*]—I will now endeavour to deal with those points which I have either imperfectly touched upon or entirely omitted to notice.

In the first place, I have to thank Mr. Leonard, of the South Eastern Railway, for having called my attention to the omission in the opening part of the paper in relation to the electric lighting carried out by his company. Mr. Langdon.

Turning to the cost of working, Mr. Fletcher, of the North Western Railway, is to be complimented upon the very satisfactory result he has so far attained at Liverpool. At the same time, we must bear in mind that these are results derived from the busiest months of the year, and that the installation has only just been laid down. Repairs are absent, but repairs are a factor which will be sure to assert itself.

I think I may say that the entire discussion has supported the views advanced in the paper in respect of the advantages and economy attending the use of the electric light for the purposes referred to. The question of cost will be found to vary to some extent in almost all instances. It must depend in a great measure upon the description of plant—whether employed for arc lighting or for incandescent lighting, the area lighted, &c.

Mr. Weekes has asked the price of coal. It may be said to vary from 8s. to 17s. per ton. In the Midlands—with the exception of Derby, where it may be taken at 10s.—the cost averages 8s. per ton. In London the cost is 17s.

Captain Sankey has been good enough to make some deductions in relation to the coal consumption at Derby, and at the Lawley Street Depôt, Birmingham. These deductions command our careful consideration. Returns of the work done, and coal consumed, show that at Derby for the half-year ending December 31st, 1894, the average consumption was 10·2 lbs. per unit, and that at Lawley Street 10·6 lbs. per unit; and Table II. gives the cost at the relative depôts. That applicable to Derby carries a charge for carting and stacking which has not to be incurred at Birmingham, where the coal is run, by means of a siding, straight into the coal bunker. The load at Birmingham is much more regular than that at Derby.

Before leaving the question of cost, I find it is necessary to say something further with respect to interest on outlay, and depreciation. Table II., as has been stated, does not include any

Mr. Langdon. charge under this head, nor does it cover taxes, or take into account the value of land on which the generating plant stands. Upon the question of depreciation I, in some previous remarks, referred to some figures in relation to the "life" which it had in certain quarters been agreed should be accorded to the various kinds of electric lighting apparatus and machinery. Subsequent information has raised a doubt if these deductions are reliable, and I have accordingly withdrawn them. As stated, personally I can see no reason for a depreciation charge, from the fact that the entire apparatus must be kept in perfect order, and repairs or renewals, as they fall in, dealt with there and then. All such charges will in this way go to the current cost for upholding and maintaining the apparatus. This course has been pursued by me, and the charges given in Table II. include for the year in question all such claims as have arisen. A time no doubt will come when the existing class of apparatus may, with advantage, be superseded by improved apparatus. But that apparatus would only be superseded by other which is more economical, or which will afford other equally good reasons for the change; and it is quite an open question whether the cost of such changes should be provided for out of current charges, or whether they should not more strictly be provided for out of that economy which their introduction is to effect. For the more complete comparison of the three cases of cost of electric *versus* gas lighting which I have given in the paper, and which made no provision for interest or depreciation, I have appended two statements showing the result, allowing in each 3½ per cent. interest on the outlay, and in the one case 5 per cent. depreciation, and in another 2½ per cent. depreciation. In these statements it is to be observed that gas is debited with nothing beyond the bare cost of the gas which would be burnt. The object of these statements is to show that where there is a reasonable amount of gas lighting the relative cost is largely in favour of the electric light, even when encumbered with a depreciation charge of 5 per cent.

If a depreciation charge is considered necessary, although a fixed sum on the outlay, it will of course each year vary per "unit" according to the output, and other moving charges.

Mr. Langdon.

Table III.

Comparative Statement showing Cost of the Electric Light employed for lighting the Ground Floors of the following Sheds (including all working charges, repairs, cleaning, &c.), with 6 per cent.—viz., $3\frac{1}{2}$ per cent. interest on capital, and $2\frac{1}{2}$ per cent. for future replacement of apparatus—in the one instance, and $8\frac{1}{2}$ per cent.—viz., $3\frac{1}{2}$ per cent. interest on capital, and 5 per cent. for replacement; as against the Cost of Gas—being for the gas burnt only—making no allowance for depreciation or replacement of gas apparatus, pipes, lamps, &c., or for interest on outlay, repairs, cleaning, &c.

Depôt.	Capital applicable to Arc Lighting.	Number of Arc Lamps in use.	Cost per Arc Lamp.	Annual Cost per Lamp at 6 %.	Lamp-Hours per Lamp per Annum.	Cost per Lamp-Hour—Interest & Replacement.	Cost of Working, as per Table II.	Total Cost per Lamp-Hour.	Number of Arcs in Shed.	Cost for Shed Arc Lamps per Hour.	Cost of Gas only for same Shed (see page 299).	Balance in favour of the Electric Light per Hour Lighting.	Total Saving per Annum.	Candle Power (Nominal). Electric Light.	Candle Power (Nominal). Gas.
At 6 per Cent.—viz., $3\frac{1}{2}$ per Cent. Interest and $2\frac{1}{2}$ per Cent. Replacement.															
	£		£	d.		d.	d.	d.		d.	d.	d.	£		
Birmingham (Central)	6,000	73	83	1,195	4,208	0·28	1·41	1·69	30	51	62	11	193	56,000	12,300
Birmingham (Lawley Street)	10,000	167	60	864	3,290	0·26	0·93	1·19	53	63	100	37	507	63,600	19,500
St. Pancras (Somers Town) ...	13,000	242	54	778	2,954	0·26	1·61	1·87	29	54	61	7	86	58,000	12,900
									Saving	per	annum	... ... £	786		
At $8\frac{1}{2}$ per Cent.—viz., $3\frac{1}{2}$ per Cent. Interest and 5 per Cent. Replacement.															
				$8\frac{1}{2}$ %											
Birmingham (Central)	As above.	As above.	As above.	1,692	As above.	0·40	As above.	1·81	As above.	54	As above.	8	140	As above	As above.
Birmingham (Lawley Street)				1,224		0·37		1·80		69		31	425		
St. Pancras (Somers Town) ...				1,102		0·37		1·98		57		4	50		
									Saving	per	annum	... ... £	615		
As against £1,050, exclusive of Interest or Replacement.															

Note.—The above statements do not include rates and taxes, interest on value of land, or carriage of coal.

Mr. Langdon.

Three and a half per cent. interest, plus 2½ per cent. depreciation on the Somers Town figures—Table II.—raises the total charge per unit from 3·22d. to 3·70d.—practically ½d. for the year 1894.

Subsequent to the discussion a question was put to me with respect to the salaries of senior officers. The total charge in Table II. includes the salaries of the resident staff, of the divisional engineers—Messrs. W. L. Preece and J. Sayers—of their personal clerk, stationery, and every item used upon the work. The charges of all depôts are kept distinct. All works are carried out under Works Orders bearing a short descriptive title and a number, and everything required for each work is asked for under the number and title of that order. Thus a very accurate means exists of conveying to each work—whether in construction or maintenance—the exact cost incurred throughout the year, or any portion of it.

I am afraid I have nothing to add to what has been said in the paper with respect to the distance between and height of lamps. To Mr. Trotter's remarks I have previously referred. He, as is well known, has devoted great attention to the question, and, personally, I cannot sufficiently express my indebtedness for the trouble he has taken in applying the data which he has established to the practice pursued by the Midland. Elevated lights of high power afford an excellent general diffusion of light, but in railway-yard working the light is required chiefly at certain points only. Unless the elevated lights are of greatly increased value—which, of course, means increased cost—they will, I fear, fail to afford the required light at these points. In goods-yard working labels on goods are not required to be read; that is done in the goods shed. The yard work is generally that of marshalling the stock, and the shunting ground and "points" are the sections calling for light. As Mr. Trotter puts it, we want a maximum at given points, and not a general minimum light spread over a large area. In the warehouses, of course, there is no question of height; you have to give plenty of light, and to arrange your lights as best you may, avoiding cranes, pillars, &c.

The lighting of the platforms of the passenger stations at St.

Pancras and Leicester is open to the observation of all, and I question if it would be improved by any alteration of the existing lights, or by the employment of lights of greater power—with, of course, due regard to economy. Mr. Langdon

Dr. Preller asks if the density of the atmosphere affects the the light. Yes; precisely as with gas. The electric light has very little, if any, more penetrative power in fog than other illuminants.

The President has advanced a reason why the Midland has so far used only 10-ampere lamps. He says: "Mr. Langdon only "used 10-ampere and smaller lamps because the plant came over "from America equipped for that size." The Midland started with the Thomson-Houston system because it was believed to be that most suitable to its wants; and I feel bound to add that, after some years' experience of this system, it, in my opinion, still possesses advantages over other systems for the description of lighting required for our purposes. It originally came from America, it is true, as did the Brush, but it is now manufactured in England by an English company. It must not, moreover, be inferred that we use the Thomson-Houston system only for arc lighting. I had thought this had been clearly stated in the paper. Experience has shown us that the 20-foot elevation is a good elevation for our purposes. It is more economical, I believe, than the 40- or 50-foot elevation; and thus the 10-ampere light gives us all we require. The 6·8-ampere lights are only used where some special reason exists for doing so.

The proposals set forth in the concluding portion of the paper have been productive, as was perhaps to be anticipated, of an expression of somewhat divergent views. Direct driving, in preference to driving by belting, &c., may, I presume, be regarded as generally approved; but when we come to the question of a form of generator that shall be applicable to all our demands, views are by no means united. Possibly it would be over-sanguine to anticipate otherwise. Mr. Manville points to the advantages of the Portsmouth system—alternator, with rectified currents—but he gives us no information on the point of efficiency, or loss between generator and the point of consump-

Mr. Langdon. tion. Mr. Albright suggests direct current on the three-wire system. He condemns high tension; and his views are strongly supported by Mr. Crompton, the President. Mr. Chamen, in advocating the same system, directs attention to what has been done by direct current at 400 volts under the three-wire system for a stretch of 5½ miles at the Victoria and Royal Albert Docks. Mr. Mordey considers it impossible to meet prospective demands by a two- or three-wire system, or otherwise than by an alternator system, possibly aided by rectification. Railway lighting differs from street lighting in that lights are turned on and off, without notice, just as required. And this must be so; in fact, electricity must give all the advantages of gas, and something more, in order to completely satisfy, not only public, but private, demands; and the system employed should be such as will meet these demands without sacrifice of economy. Is a three-wire system best destined to do so? We know that lights in series-parallel, if turned out in part only, entail a loss or waste of current to the extent of the current consumed by the lamps turned off. Experience tells us that, plot out our lamps as we will, exigencies of traffic are ever demanding changes, and that that which is entirely satisfactory to-day, and productive of economy, may, owing to these demands, to-morrow call for rearrangement. It is true the loss is merely loss of power — that you are running so much more horse-power than you ought for the work being done. This on a fairly full plant may not be, and probably is not, a great question, but still it is there.

With a transforming and rectifying system the loss would probably be as great. There is not, therefore, from this point of view, any great disadvantage between the two methods.

But is this the controlling factor? Can we fix a limitation to our prospective requirements? and do these systems come within that limitation? Is there a practical limitation in area, in distance, to the three-wire system? I cannot help thinking much must depend upon the answer our knowledge of the subject, or further developments, may bring to our mind. Limitation of area or extension must, I think, prove the main factor. These

are, I pray it may be understood, but my own views, and no greater weight should be attached to them. Mr. Langdon.

Apart from the question of the form of generator which may be most advantageously employed for future works of magnitude, is the important point raised by Mr. A. Preece in relation to the cost of the output at Holloway. He tells us that their expenses are higher than they would be were it not that they are working under two departments—one in charge of the electrical gear, and the other in charge of the mechanical apparatus. This is not an isolated case. In the past no doubt it was necessary that one department only should be responsible for the mechanical machinery; but there was then no other department using mechanical machinery to such an extent as it is likely to be used, and is, in fact, now being used, for electrical purposes. These electrical demands are creating another engineering department, and that of a somewhat special character. There can be little doubt it will be found politically and economically desirable that this department shall be entirely responsible for, and shall, to that end, have complete control of, all that is needful to ensure the most useful and economical results. This will only be accomplished by obliging the responsible department to dominate its entire arrangements. It is, of course, proper that where work can be done by other departments of the company it should be done, but this should not interfere with the staff arrangements or the duty sheet. These should be in the hands of the man who is responsible for the production of that for which the machinery is laid down.

Attention has been drawn to the question of train-lighting, and in one or two instances surprise has been expressed why it has not been referred to. The paper deals with work in operation on the Midland Railway; and, although I am aware the electric light is employed for many trains on the Brighton line, and is in use, to a partial extent, on the Great Northern, on the South Eastern, and on the Great Northern of Ireland, unacquainted with the details or results attained in these instances, I have felt it would be more fitting for others to deal with this branch of the subject than to introduce it into this paper. The experimental

Mr. Langdon.

trains on the Midland have now been discontinued for some time. Although I could not at the time regard the success attending these trials as sufficient to enable me to recommend the extension of the system to the entire stock, I cannot look upon them as a failure. Further time could not, however, be sacrificed. It was felt that the adoption of a better form of lighting than that afforded by the old oil lamp was so urgent that it would not admit of further delay; and, although the electric light as against gas had many admirers, manifestly two systems could not economically be sustained; and, as a portion of the company's stock was already lighted by gas, and certain depôts for its production already existed, it was decided gas should be adopted. Nevertheless, if I may here express an opinion, my confidence that the railway trains of all large companies will eventually be lighted electrically, remains as steadfast as ever.

Dr. Preller has, in reference to the instructions drafted by me for the avoidance and treatment of accidental shock from electrical current, pointed out that the system which depends upon the traction of the tongue is due to D'Arsonval, and not Laborde. These instructions were compiled by me some time prior to anything of the kind having come under my notice, except some very—and purposely so—condensed instructions for first aid which appeared in the *Lancet* from the pen of Dr. Hedley, of Brighton. It was from one of the publications from which I was collecting data for this treatment that I obtained the reference to "Laborde;" but, subsequent correspondence having in all cases which I have noticed quoted "D'Arsonval," I readily accept Dr. Preller's courteous correction; and it is with much pleasure I avail myself of this opportunity to tender my thanks to Dr. Hedley, of Brighton, for having, with much kindly courtesy, perused the draft of that portion of the instructions referring to the *treatment* of patients. That portion of these instructions was compiled from various sources—very much from the columns of the *Electrical Review*. That portion devoted to the steps which should be taken to avoid accidents is, however, in my opinion, even more important; and I venture to hope that—modified as may be considered suitable to the requirements of

the system to which they may be applied—they may be generally adopted. Manifestly it is better to avoid accident than even to successfully treat it when incurred. Moreover, we have painful proof that under certain conditions recovery is not possible. There is therefore the greater need for the adoption of regulations which may tend to prevent such disastrous consequences. Mr. Langdon.

The President, in his concluding remarks, has directed attention to the important part which the electric light—as a large light—plays in the saving of life in busy goods yards and shunting grounds. The knowledge of this advantage is not peculiar to the authority quoted by the President. It is, I believe, well known and recognised by all who have any knowledge of the advantages of the electric light as an important reason for its employment at such points. It is not in my power to quote figures, but I am in a position to say that, so far as the Midland stations are concerned, it has been of service in this respect, and that this has been fully recognised.

With reference to Mr. Dowson's communication, I do not propose entering upon a discussion of the gas-motor question. If it is in my power at a future date to produce data in relation to the Leicester installation, I shall with pleasure bring that data before the members of the Institution. At the present moment I cannot regard the arrangements of the Leicester installation as normal. Any figures which I might at the present moment produce would not be so satisfactory, or so reliable, as those which may be forthcoming when the work is complete and the staff arrangements finally settled.

The Secretary has handed me a communication which he has received from Mr. F. W. Webb, of the London and North Western Railway. That communication I have returned to the Secretary for publication with the paper, and I have pleasure in affording the following replies to Mr. Webb's queries :—

1. Repairs or renewals required during the period in question are charged to the current expenditure.
2. Carriage is not included in the charge made for coal, nor is any charge made for waggon hire or for carriage of stores between the telegraph headquarters and the

Mr. Langdon.

depôt at which it is required for use; but where stores are ordered from contractors the carriage is paid to point of delivery.

3. No; neither interest on capital, on land, buildings, or plant, are, as has been stated, included; nor are rates paid on local assessment taken to account. Whether this is, as Mr. Webb puts it, "a serious omission," I must leave to the decision of others. The statement was put forward as "the output and cost of *working*" (see preceding paragraph to Table II.), and was never advanced as embracing interest on capital outlay; and I would submit that this is fairly clear from the details which go towards making up the sum total of the total charge.

4. The table prepared by me was not prepared for the purpose of comparing with the charges of public supply sources, although the general result admits of such comparison. Clearly, *if the total cost is less per unit than that at which the current only is obtainable*, the object in view, viz.—to show *that it is more economical to generate from one's own plant* than to obtain the current from outside sources—is attained. I am not prepared to give the cost of current separate from that of other charges; nor do I think it, under the circumstances, at all necessary.

5. Incorporated with my remarks is a Table (III.) showing interest and depreciation, in addition to other charges, produced for purpose of comparison with gas. I am sorry if this does not meet Mr. Webb's requirements.

6. This has been dealt with in my remarks.

Mr. Webb has been good enough to check the figures in Table II., and to call attention to some slight miscalculations, for which I am much obliged, and which have been adjusted.

Mr. Webb has, finally, been good enough to furnish a statement of the cost of the London and North Western lighting at Broad Street Station, as compared with that for Somers Town, December, 1894, half-year. The comparison

between these two stations is scarcely on the same level. Somers Town is solely arc lighting; Broad Street appears to be mainly incandescent lighting. The labour charge for arc lighting is, of course, much in excess of that for incandescent lighting. The Broad Street plant would appear to be new, whilst that at Somers Town has been in use for some years. Coal at Somers Town costs 17s. per ton; Mr. Webb has not favoured us with his cost in this respect. The result obtained by Mr. Webb appears, so far as one can judge—for I am not clear if it includes carbons and carboning—most encouraging. Mr. Langdon

[*May* 6*th.*]—Although my rejoinder to the discussion has been completed, and is ere this in the hands of the printers, and the insertion of Mr. Esson's remarks (which have been kindly forwarded to me by the Secretary) may entail inconvenience, and possibly some delay, in the issue of the Journal of the Institution, I have requested, having regard to the tone in which the communication is couched, that it may be included, as its exclusion might possibly be attributed to an undesirable reason. At the same time, I feel it due to myself to observe that the object of the paper which the Institution has done me the honour to accept was not so much the comparison of figures, in the sense in which Mr. Esson advances those put forward by him, as to show what had been done, and at what cost. Naturally, I was quite prepared to find that the Midland results might be "beaten," or not; that, however, was not the object of the paper. If the Midland figures are *bonâ fide* "beaten," so much more are the views advanced by me in relation to the electric light enhanced.

I do not quite understand why the data given by Mr. Esson should not have come direct from Mr. Neale, who was, I believe, provided with an advance proof of the paper purposely that he might be in a position to furnish any such data; and, as the installation is under his supervision, it seems to me that it would have been more in order had the figures come from him.

The figures produced are based upon the three first months of the year—presumably the three months which would give the largest output, and consequently the minimum cost per unit.

Mr. Langdon. I believe I am right in saying that the installation referred to has been in operation some time. If so, it would have afforded a more reliable result had the data been extended over a greater period.

Dealing with the figures as they stand, I append the following remarks:—

1. The incandescent lighting is, in proportion to the arc lighting, nearly as three to one. (With incandescent lighting there is, of course, much less labour, material, and repair than with arc lighting.)

2. Coal is possibly cheaper than on the Midland; if not, the result for a variable load is remarkably good.

3. Interest and depreciation: The amount allowed under each head is not stated. Taking it at 3½ per cent. interest and 2½ per cent. depreciation, it would appear that the cost of the entire installation did not exceed £3,000—buildings, boilers, engines, wiring, &c.!

4. Total, 1·898d. per unit, is a result for a mixed installation which, even for the best three months of the year, I am under the impression has not been achieved elsewhere.

Possibly this may be accounted for by the load, which, for the installation in question, appears to be unusually heavy. 2,785 units per week would mean—seven days to the week—398 units per day. Twenty-four arcs, consuming 500 watts each per hour, and working 12 hours per days (average for three months), would absorb 144 units per day. This leaves 254 units per day for the glow lamps. At Derby, the headquarters of the Midland, the consumption of current on an equivalent 4,000 lights, serving offices, waiting-rooms, refreshment rooms, &c., night and day, was but 77,740 units for the December half-year—an average of 2,990 units per week, or 427 per day, as against the 254 units for the 1,100 lights at Stoke. Of course the 2,785 units per week is the main factor which governs all the results; reduce it and we raise the cost per unit under each head.

The President: I have to inform you that this is the last time we shall enjoy the privilege of meeting in this hall for a

considerable period. The structural alterations of the establishment now demand that the whole building should be given up to the contractors, and the Institution of Civil Engineers have found it necessary, therefore, to close (only temporarily, I trust) their hospitable doors to us. We have not yet decided as to where the remaining meetings of the session will be held, but hope to be able to make the announcement in a few days.

I now beg to report that the scrutineers certify that the following candidates have been duly elected :—

Member :

Alfred Henry Walton.

Associates :

Alfred Henry Avery.
Charles J. Bosanquet.
Thomas William Broadbent.
Cadwallader M. van Cuylenburg.
Richard McGillivray Dawkins.
Frederick Arthur Glover.
John Peal Nelson.
Louis Foley Noakes.
Stuart Richardson.
William Walker.
Arthur Joseph Clark Waterland.

Students :

Norman Clough.
Walter Daniell.
Alfred Milligan.
William Slater Naylor.
L. Nieuwenhuys.
Neville James Payn.
Reginald B. Roberts.
Ernest N. Ruddock.
Ernest Claud Short.

ABSTRACTS.

RICCARDO MALAGOLI—AN ALTERNATING ROTARY MAGNETIC FIELD, AND ITS APPLICATION.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 1, *p.* 1.)

1. The author defines an alternating rotating vector, as one whose direction rotates uniformly round an axis, and which in making a complete revolution passes through all the sinusoidal values. The vector has consequently its maxima and zero values 90° apart.

2. The alternating rotating vector may also be looked upon as an ordinary alternating vector, having such an angular velocity to the right or left that the vector makes a complete revolution during one period of alternation.

Professor Ferraris has shown that an alternating vector of fixed direction may be considered as the resultant of two constant vectors the amplitude of which is half the maximum amplitude of the alternating vector, and which rotate at such a velocity in the opposite direction that each makes a complete revolution for one period of alternation. The angle between the two vectors at the origin must be equal to or double that of the phase.

The alternating rotating vector may then be considered as the resultant of two equal and constant vectors, one turning to the right and the other to the left, and having also a common motion of rotation towards the right or left according to the direction of the alternating vector.

One of the two vectors having thus two equal and opposite angular velocities will maintain a fixed position, and the other will rotate at double the velocity and in the same direction as the alternating rotating vector.

3. If O A be the constant fixed vector, O B a position of the moving vector, round a point o with a frequency n, the resultant vector, O C, can be determined as a function of the time by the relation,

$$O\,C = 2\,O\,A \cos n\,\pi\,t,$$

if the origin be taken at the time that the vectors are superposed; or, more generally, by

$$O\,C = 2\,O\,A \cos (n\,\pi\,t + a),$$

if $2\,a$ be the angle formed by the two constant vectors at the origin of time.

While the vector O B describes a complete circle the resultant vector describes a semicircle, passing through all positions above the origin line.

Magnetic fields which can be definitely represented by an alternating rotating vector are susceptible of many applications, of which the following seem the most important:—

• If a ring of soft iron be covered with a uniform winding similar to that of a Gramme armature, with connections made at points, A and B, diametrically opposite to one another; then if an alternating magnetic field rotates round this ring, towards the right, for example, so that the zero values coincide with A and

B: then, for instance, a magnetic north pole will exist on one side of the ring having its zero values at A and B, and its maximum value at a point, C, midway between them. The turns of the coil at the point C are the seat of an E.M.F. constant both in sense and value during the whole period of the magnetic field; while those to the right and left of C have weaker E.M.F.'s the nearer they approach A or B, but so adjusting themselves that the displacement of the pole and its pulsation give rise to a current of fixed direction, tending to produce, according to Lenz's law, a polarity at A and B opposite to that developed by the alternating rotating field.

The coils of the two halves of the ring are thus the seat of opposing E.M.F.'s, and there will exist between A and B a constant difference of potential. A continuous-current machine could be constructed having two magnetic fields, one rotating and the other fixed, to be either self or separately excited. This machine would have no commutator. A transformer for converting alternating into continuous currents might also be designed on the above principle, and would possess the advantage of having no moving parts.

M. D. FARMAN—ELECTRIC DISINFECTION BY THE HERMITE PROCESS.

(*Bulletin de la Société Internationale des Electriciens*, *Vol.* 12, *No.* 115, *p.* 51.)

This system depends on the electrolysis of sea water, or of a suitable mixture of chloride of magnesium and chloride of sodium.

The disinfecting power of electrolysed sea water, called "hermitine," is greater than that of other known disinfectants, and its cost of production is lower. The action which takes place during the electrolysis of a solution containing different salts has been described by M. Ostwald in the following manner:—

Consider that the salts are partly decomposed, and that their atoms move in the electrolyte at a velocity proportional to the ohmic resistance of the electrolyte.

These free atoms in moving rapidly to and fro would carry electric charges from one electrode to the other. The atom would only fix itself to the electrode under a certain difference of potential existing between the charge of the atom and that of the electrode. This difference of potential, capable of destroying the motion of the atom, would be called the electro-motive force of polarisation. In order to confirm this hypothesis, Ostwald made the following experiment:—

Two beakers, A and B, containing K Cl, were connected by a syphon. A negatively charged body was brought near A, and then withdrawn. It was then found that hydrogen was liberated at A through the decomposition of water due to an excess of potassium. The electrolyte contained free atoms of potassium which were displaced from B to A under the influence of an electric charge. This theory explains the decomposition of two salts; the electro-positive ions of one of the salts will neutralise the electro-negative ions of the other, and the products of combustion will only appear at the electrodes on condition that the E.M.F. employed be great enough. It also explains how without decomposition the electric charges may be transmitted by means of an electrolyte.

Sea water under these latitudes has the following composition:—

			Electro-motive Force of Polarisation.
$Ca\,SO^4$	=	1·392 gr. per litre	6·90 volts.
$Mg\,SO^4$	=	2·325 „ „	6·5 „
$Mg\,Cl^2$	=	3·668 „ „	3·3 „
$Na\,Cl$	=	27·7 „ „	4·3 „
$H^2\,O$	=	965·0 „ „	1·48 „

$Mg\,Cl^2$ and $H^2\,O$ are decomposed to obtain nascent chlorine and oxygen; but, in order not to decompose the chloride of sodium, which acts solely as a conductor, M. Hermite adopted a pressure, E, greater than the sum of the electro-motive forces of compounds which would be decomposed before the Na Cl, and smaller than the sum increased by the E.M.F. of decomposition of this latter salt.

$$E = \Sigma e + i r.$$

An electro-motive force of 6 volts was adopted for 1,000 amperes.

$$6 \text{ volts} = 4{\cdot}78 + 1{,}000\, r;$$

$$r = \frac{1{\cdot}22}{1{,}000} = 0{\cdot}00122 \text{ ohm.}$$

This very low resistance is due to the presence of chloride of sodium, although this salt takes no part in the electrolytic action. This fact, discovered by M. Hermite, is of great commercial importance in the manufacture of the hypochlorite, as it greatly reduces the cost of the electrical energy necessary to decompose the chloride of magnesium. The Cl^2 and O combine at the positive electrode, and dissolve as 2 (Cl O H).

In practice it is found that 0·9 ampere-hour is necessary to produce 1 gramme of Cl, notwithstanding the inevitable losses of gas. Under practical working conditions 6 watt-hours are necessary for the production of 1 gramme of chlorine.

The electrolysed water is a perfect and almost instantaneous deodoriser, and for equal volumes has even a greater antiseptic power than corrosive sublimate.

Electrolysed sea water containing a hypochlorite has strong bleaching properties, and, from an industrial point of view, has the following advantages over other bleaching agents:—That the liquor can be used almost indefinitely; that it is absolutely neutral, and owes its bleaching action to a hypochlorite, and not to a mixture containing an excess of a base which is harmful to the materials to be bleached.

The apparatus for the production of hermitine is very simple. The C type portable plant is capable of producing 250 grammes of chlorine per hour. The vat contains two spindles carrying a series of zinc discs receiving motion from two endless screws. Between each disc are placed four glass rods coated with platinum foil. The whole is submerged in sea water. The platinum-covered rods are connected to the positive pole of the dynamo, while the zincs are connected to the negative pole. Platinum was found to be the only practical substance which would resist the action of nascent chlorine and oxygen.

Two reservoirs receive the liquor after passing through the electrolyser, and a pump is employed for maintaining a constant level of liquid in the vat.

The dynamo is capable of giving 2,500 to 3,000 amperes at 6 volts.

In order to employ electrical energy at ordinary supply pressures, M. Hermite has designed a special apparatus consisting of several small electrolysers connected in series, and which could be used in any building supplied with electricity.

The cost of this process would work out as follows:—Say 1 gramme of chlorine requires 10 watt-hours: then 1,000 grammes would require 0·48 H.P. for 24 hours. Then, taking the cost of 1 H.P. for 24 hours at 1 franc, 1 kilogramme of chlorine in the form of hypochloric acid would cost 0·50 franc at a seaside town.

According to experiments made at Ipswich, lasting over two months, it was found that the deodorising of the drains would cost 0·90 franc per inhabitant per annum, and approximately three times this figure to obtain complete sterilisation. After several years' experience, electric bleaching has shown an economy of 50 per cent. over other bleaching processes, and has yielded better results.

R. V. PICOU—THE TRANSMISSION OF POWER BY SYNCHRONOUS ALTERNATING-CURRENT MOTORS.

(*Bulletin de la Société Internationale des Electriciens, Vol.* 12, *No.* 115, *p.* 60.)

The author refers to the fact, first published by Mr. Mordey, that in transmitting power through a synchronous motor it is possible to reduce the current to a minimum by varying the excitation.

The curve of current plotted as a function of the excitation is V-shaped, which becomes most accentuated when the power transmitted is small as compared to the normal load of the motor. Mr. Kapp has also published similar curves with corrections made for armature reactions. Professor Silvanus Thompson has shown that, for the maximum values of excitation when the current has passed its minimum value, the latter is so in advance of the E.M.F. of the source that its action is comparable to that of a condenser.

The method employed by the author for studying the various conditions of transmission is developed from Blakesley's well-known graphical method.

A 20-kilowatt transmission at 2,000 volts is treated by this method. It is found that the normal current of 15 amperes may be varied between 10 and 16 amperes at full load, and the motor will work with an advance or lag of 45 degrees.

Under these conditions the E.M.F. of the motor will vary between 2,186 and 1,655 volts.

A V-shaped curve is also obtained by plotting the current as a function of the volts. A set of such curves are given corresponding to different outputs of the motor. In each case the difference of phase is limited by the maximum current which the motor will stand. A line drawn through the minima of these curves is inclined at 135 degrees to the horizontal. In order to ensure the minimum current at all loads, the excitation will only therefore have to be varied through small limits.

In order to simplify the questions relating to excitation, it is necessary to carefully analyse the effects due to difference of phase on the armature reactions.

When in a machine the current and E.M.F. are in phase with one another,

both will be zero at the same instant, at which time the flux is at its maximum and the pole-pieces are opposite one another. The action of the main current on the magnetic circuit at this moment will be zero.

If the current lags, it will not have reached its zero value at the moment when the pole-pieces are opposite one another. The result will be a decrease in the excitation of the generator, and an increase in that of the motor. If the current is in advance, the opposite reactions will then take place.

It is then seen that the variation in the excitation of a motor, to pass from the maximum current in advance to the maximum lagging current, is much greater than that corresponding to the variation in electro-motive force alone, irrespective of armature reaction.

A graphical method is given to determine the most advantageous value to give to a constant excitation. A comparison is made between synchronous and non-synchronous motors;—the author disputes a conclusion arrived at by M. Em. Kolben in *L'Industrie Electrique* of January 5th that non-synchronous motors are more efficient than synchronous motors.

J. CAURO—THE ELECTROSTATIC CAPACITY OF COILS, AND ITS INFLUENCE ON THE MEASUREMENT OF COEFFICIENTS OF INDUCTION BY THE WHEATSTONE BRIDGE.

(*Comptes Rendus, Vol.* 120, *No.* 6, *p.* 508.)

Coils with a double winding have a negligible self-induction, but their capacity, which increases considerably with the length of wire, may under certain conditions become considerable. M. Chaperon proposed an alternating winding, in order to annul self-induction and to considerably reduce capacity.

Good results were obtained by a method devised by the author, which consists in winding the alternating layers from the same point, and returning by means of a straight wire.

A Wheatstone bridge was employed to measure the effects.

If α, β, ω, χ, be the fictitious variations of resistance corresponding to the E.M.F. produced by induction and capacity, a be a standard German silver resistance having negligible self-induction and capacity, p the coil under test, q a carbon resistance approximately equal to that of the coil, b an adjustable resistance (a and b are primarily equal, and very low as compared to p and q), then the following relation is obtained:—

$$\alpha = \frac{a}{b} + \beta \frac{a}{-} \omega - \frac{a}{q} \chi.$$

When comparing approximately similar coils, the author found that the errors due to capacity might be considerable with the ordinary double winding, that they were diminished with the Chaperon winding, and that they were further reduced with the improvement suggested above. The following are the results of experiments made on three bobbins of copper wire 0·17 mm. in diameter:—

Description of Coil.	R.	No. of Turns.	a.	C.	ω deduced.
Ordinary winding	13,287 ω	9,380	187	0·72 ϕ	− 1·79
Chaperon alternating winding	14,770 ω	9,380	187	0·10 ϕ	− 0·27
New alternating winding ...	14,890 ω	9,380	187	0·06 ϕ	− 0·16

The ordinary winding was thus found to have a negative coefficient of self-induction $L = -17 \cdot 9$. By employing the double winding and taking mutual induction into account, $L = 0 \cdot 13$; thus obtaining an error 12 times greater than the one to be corrected.

The author also obtained negative self-inductions with German silver coils having a resistance of about 66,000 ω. By introducing soft iron into the bobbin the apparent self-induction could be annulled and L made zero; and, conversely, by employing coils of 3,000 ω having a positive coefficient of self-induction and immersing them in petroleum the apparent self-induction could be made negative.

In conclusion, the author considers it only necessary to employ coils with a double winding when the resistances are low; with high resistances an error due to capacity is introduced greater than the one it is necessary to avoid. In the latter case it would be necessary to employ the Chaperon winding or that suggested by the author. Capacity is greatest in the case of ordinary winding, and may become preponderant if the resistance be great enough, and thus tend to give a negative self-induction. These effects due to capacity are negligible when the tests are made with an ordinary Wheatstone bridge, and would only become apparent in coils having low coefficients of self-induction and high resistances.

ANON.—SINGLE AND MULTIPHASE ALTERNATORS OF THE GENERAL ELECTRIC COMPANY OF BERLIN, DESIGNED BY M. V. DOLIVO-DOBROWOLSKY.

(*L'Éclairage Électrique, Vol.* 1, *No.* 8, 1895, *p.* 364.)

It is at times difficult to design an alternator on the most economical lines, owing to the large number of poles necessary for a comparatively high frequency and low angular velocity.

A considerable advantage is obtained by employing one magnetising coil only; and, by suitable designing, this coil need not rotate with the magnets. The two types of alternators now being constructed by the General Electric Company of Berlin have a single fixed magnetising coil.

One design is specially adapted for high-speed machines to be rope- or belt-driven.

The magnetic circuit consists of an outer frame having two sets of laminated sections fixed to either side of it and pointing radially inwards. Between these sections is placed the single magnetising coil. The magnetic circuit between the two sets of armature sections is completed by the rotating inductor, consisting of U-shaped castings fixed to radial arms. Machines of an analogous type are constructed by Stanley, in America, and by Sollmann, of the firm of Wahl & Co., in Russia.

The above company have constructed a series of machines of this and of a modified type.

A 100-H.P. multiphase generator of this design is working at Schwenger's sugar refinery at Werdingen.

A section of a polyphase machine is shown in which the armature has three times as many projections as there are magnet poles; thus obtaining three currents differing in phase by 120°.

Another method for polyphase winding consists in crossing the armature coils, consisting of single bars of copper—a method specially adapted for large machines. Three machines of this type of 300 H.P. are in course of construction. The second type of alternator is specially designed for direct coupling.

The magnetic circuit consists of a fixed C-shaped casting containing the magnetising coil. The polar faces are laminated, and divided into a suitable number of sections to receive the armature coils. Between the polar faces rotates a laminated inductor, fixed to the fly-wheel of the machine. The company have in course of construction for the Strasburg central station five polyphase machines of this type—three of 400 H.P., and two of 200 H.P.

These machines have no brush contacts, and are so designed as to offer ready access to either field magnet or armature. These machines have a very small drop of potential between 0 load and full load. The hysteresis loss is small; and, owing to the small amount of power required for excitation, these machines have a high efficiency at all loads. The following are a few particulars of the 400-H.P. machine destined for the Strasburg station:—

Power: 280 kilowatts at 2,750 volts.
Angular velocity: 150 revolutions per minute.
Number of poles: 40.
Weight of copper on the field coil: 730 kilogrammes.
Weight of copper on the two armatures: 200 kilogrammes.
Excitation: 4,000 watts = about 1·4 per cent.
Power lost in the two armatures: about 2 per cent.
Total hysteresis loss = about 1 3 per cent.

The machine has only about 2 kilogrammes of copper per horse-power. The ratio of the ampere-turns of one armature coil to those of the exciting coil is 1 : 15.

The following are the weights of iron:—

	Kilogrammes.
Carcass	6,000
Two armatures	2,000
Pole-pieces	750
Total weight of iron, excluding spindle and bearings	8,750

The article is concluded with remarks as to the best "drop" to give to alternators. This effect becomes the most objectionable in the running of motors, where armature reactions become greater than when running on lamps. A machine with a drop of 15 per cent. with the latter would not be suitable for running motors. The "drop" should not exceed 4 to 5 per cent. with lighting currents, which corresponds to 15 or 20 per cent. for motors. The only objection which could be raised against such machines would be the danger of short-circuiting—a risk which is no greater than in the case of continuous-current machines.

M. FARMAN—NOTES ON THE THEORY OF DYNAMO MACHINES.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 8, *p.* 318.)

The author first considers influence of the dimensions of the air gap on the design. As the air gap is the preponderant resistance in the magnetic circuit, it would seem advisable to reduce this as much as possible.

Experience has shown that a small air gap necessitates a large lead for the brushes, and not a proportional increase in the E.M.F. A decrease in the air gap will under certain conditions produce a drop in E.M.F., due to armature reaction.

If a be the angle of lead given to the brushes, n I the armature ampere-turns, and m I the exciting ampere-turns, then

$$4\pi\left(m i - \frac{1}{2}\frac{n}{360}a\,\mathrm{I}\right) = \Sigma\,\mathrm{N}\,\mathrm{R}_{\mathrm{M}}.$$

This formula shows that although R, the resistance of the air gap, may be decreased, N, the total flux, may not be necessarily increased, as a would increase proportionally. But apart from these reactions there is another effect which would tend to produce an apparent increase in resistance, viz., the obliqueness of the lines of force in the air gap.

If F be the field due to the electro-magnets, and F_1 the cross field, then these two values will combine to give a resultant R making an angle γ with the direction of F. The flux will then cross the air gap obliquely. The magnetic resistance will then correspond to an increase in the air gap of

$$dx = x\,\frac{\mathrm{I} - \cos\gamma}{\cos\gamma}.$$

It would therefore be necessary to find the value of x to make R a minimum, which would necessitate the solution of an equation of the third degree, of the form,

$$x^3 + 3\,b\,x^2 + (b^2 + 2\,b)\,x + b^3 = 0;$$

and the values of certain constants would have to be ascertained from machines of a similar type to the one to be designed.

R. PICTET—INFLUENCE OF LOW TEMPERATURES ON THE ATTRACTIVE POWER OF PERMANENT MAGNETS.

(*Comptes Rendus, Vol.* 120, *No.* 5, *p.* 263.)

The experiments were made on a magnet weighing 493·5 grammes, made up of three horse-shoe magnets. After magnetisation, it was made to carry its armature for two years, and was found to lift as much as 4,275 grammes. It was then left for 11 years without its armature, and then only carried 3,226·5 grammes.

The magnet was placed for these experiments in a bath of alcohol, which could be maintained at various temperatures; the attraction between the magnet and its armature being measured by means of an accurate balance.

The following are the results of four series of observations:—

Temperature of Magnet.	Force of Attraction.	Temperature of Magnet.	Force of Attraction.
+ 30	57·31	− 30	65·35
+ 20	58·48	− 40	66·70
+ 10	59·81	− 50	68·15
± 0	61·04	− 70	71·12
− 10	62·42	− 90	74·18
− 20	63·93	− 105	76·64

J. REYVAL—BOILER TESTS MADE AT THE FRANKFORT EXHIBITION.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 5, *p.* 207.)

A specially selected committee of experts was selected for the purpose of making a series of tests on the boilers at the Frankfort Exhibition. The leading results of these experiments were given in a paper read by M. O. Knaudt before the German Society of Engineers, and of which the following is a short analysis:—

The firm of E. Willmann, of Dortmund, and Durr & Co.'s Tubular Works, of Rating, exhibited two water-tube boilers with a front chamber, and the tubes closed at the far end.

The firm of Hermann & Schunmelbusch, of Kaiserslautern, exhibited a boiler with vertical tubes on the Hohlfeld system.

The firm of Goehrig & Leuchs showed a boiler with tubes rising from back to front.

The firm of Schulz-Knaudt, of Essen, exhibited a boiler with lateral tubes.

The trials on each boiler consisted of a preliminary test, followed by two final tests.

The steam passed directly into the service mains of the Exhibition, and was found to be practically dry. The Westphalian coal employed developed about 7,600 calories per kilogramme. Each kilogramme of steam generated corresponded to 640 calories.

The amount of steam produced per square metre of heating surface per hour varied between 13 and 16 kilogrammes for water-tube boilers, and was about 25 kilogrammes for fire-tube boilers without lagging, and about 17·5 with lagging.

In order to obtain comparative results for the consumption of steam, calculations were made as to the number of kilogrammes of steam generated per hour per cubic metre of water and per square metre of water surface.

All fire-tube boilers gave results about twice as favourable as with water-tube boilers, although the former produced as much as 25 kilogrammes of steam per hour per square metre of heating surface.

With regard to efficiency, or the ratio of the calories in the steam to the theoretical calories of the coal, the fire-tube boilers gave 79 per cent. efficiency, and 73·5 per cent. without lagging; the water-tube boilers gave from 73·5 per cent.

to 62 per cent. It was found that water-tube boilers were in all respects inferior to fire-tube boilers.

To burn 1 kilogramme of coal requires, theoretically, 8·5 cubic metres of air. Taking this value as unity, the tubular boilers were found to require 1·25, and ordinary cylindrical boilers from 1·6 to 1·8.

M. EDM. VAN AUBEL—ON THE ELECTRICAL RESISTANCE OF A FEW NEW ALLOYS.

(*Journal de Physique, Vol.* 4, 1895, *p.* 72.)

The following particulars of some new alloys manufactured in Germany were supplied by the manufacturers, the tests having been carried out at the Charlottenbourg Technical Institute:—

1. *Kruppine.*—This alloy, for use in electrical resistances, is manufactured at Krupp's Steel Works, in Prussia.

In the first series of experiments the wire was coated with a layer of gum-lac and heated during several hours at 150° C., then for two hours at 200° C.

Specific Gravity.	Specific Electrical Resistance at 18° C. in Microhms.	Mean Temperature Coefficient.	
8·107	84·7	Between 18° and 50°	+ 0·000791
		,, 18° ,, 100°	0·000766
		,, 18° ,, 150°	0·000749

In the second series of experiments the wire was previously heated during several days at 170° C.

Specific Electrical Resistance at 20° C. in Microhms $-\frac{\text{Cm.}}{\text{Cm.}^2}$.	Mean Temperature Coefficient.	
85·5	Between 25° and 73°	0·00076
	,, 73° and 123°	0·00069
	,, 123° and 154°	0·00066

The following table is a comparison between kruppine and other alloys used for resistance:—

Metals.	Specific Resistance in Microhms-Centimetres.	Temperature Coefficient.
German silver	20·76 (at 0°)	0·00044
Manganine...	34·0	Almost nil + 0·000008 to + 0·000018
Constantan...	50·0	Almost nil
Rheotan ...	52·5 (at 0°)	0·00041
Manganese steel	75·0 (at 15° C.)	0·00136
Nickel iron	78·3 (at 0°)	0·00093
Kruppine ...	85·5 (at 20° C.)	0·0007
Liquid mercury	94·34 (at 0° C.)	0·00072

As is seen from the above, the new Krupp steel has a very high specific resistance.

An important property of this alloy is that its temperature may be raised to

600° C. without its undergoing the slightest change, which is far from being the case with most of the alloys used for resistances.

Kruppine has a tensive strength of 60 kilogrammes per sq. mm. It is manufactured in wires varying in diameter from 0·5 mm. to 5 mm., or in strips of the following thickness: 0·5 mm., 1 mm., 2 mm., 3 mm., 4 mm.

2. The alloys manufactured by MM. Fleitmann, Witte, & Co. at Schwerte (Westphalia) were tested in the form of wires. The samples were kept at a temperature of 130° C. for 24 hours. The resistance of the wire at different temperatures was measured by placing the samples in a petroleum bath, and by means of the Wheatstone bridge.

The very high specific resistance and low temperature coefficient will be seen from the following results:—

Sample.		Resistance in Microhms of a Wire 1 cm. long, 1 sq. cm. section, at 20° C.	Variation in Resistance per Degree Increase in Temperature.
Patent alloy for extra high resistances	hard	53·1	− 0·000029
	soft	50·3	+ 0·000059
Patent Ia Ia alloy	hard	50·2	− 0 000011
	soft	47·1	+ 0·000005
Patent nickeline alloy I ...	hard	43·6	− 0·000076
	soft	40·7	+ 0·000077

The Ia Ia sample has a higher specific resistance and a lower temperature coefficient than manganine adopted by the German Physico-Technical Institute for standard electrical resistances. It would be necessary to ascertain whether this alloy is free from thermo-electric effects when in contact with brass, as was found to exist with Constantan.

P. MARCILLAC—ELECTRIC WAR SIGNALS.

(*L'Éclairage Électrique*, *Vol.* 1, 1895, *No.* 6, *p.* 255.)

The author describes the Kaselowsky system of night signals, which has been in practical use for some time past in the Italian Navy. The system allows of rapid signalling, and consists of 20 combinations of white and red signal lights, 18 of which represent the consonants and the others refer to either of two codes. The letters are signalled in quick succession, and must be repeated by the ships receiving the message in order to obviate errors.

The apparatus for sending all these signals consists of eight lights (four red and four white) and a transmitting board.

The lights are placed in a vertical line at distances of at least 1 metre from one another, the white being placed above and the red below. Each light is connected by a double wire to the transmitting board, the latter being placed at the foot of the mast or any other convenient place. On this board are painted, in two columns, the above combinations. At the side of each combination is placed a switch for lighting or extinguishing the lights.

ANON.—THE RIGHI IDIOSTATIC ELECTROMETER.

(*Comptes Rendus, No.* 6, 1895, *p.* 275.)

This new electrometer is made up of four sheets of aluminium, fixed horizontally, and parallel with one another. The two outside ones consist of fixed discs, between which are placed a third fixed disc with two holes cut in it, thus forming two sectors of 60° each, and opposite to one another, and a movable needle supported by a bifilar suspension, and placed at an extremely small distance from the sectors.

To the underside of the needle, and in a line with the suspension, is fixed a platinum wire dipping at its lower end into concentrated sulphuric acid, which serves several purposes—viz., to keep the instrument quite dry, to form a connection between needle and sectors, and to damp the oscillations of the needle. The deflection of the needle is read by means of a spot of light fixed to the platinum wire. The whole of the suspended part weighs only 1·2 grammes.

The lower disc is placed as close as possible to the sectors, but the top disc is so fixed that its distance from the needle can be varied from a very small amount to about 2 cm.

The two outer discs are connected together and to the outer case of the instrument. This arrangement forms a condenser, of which the outer discs form one plate and the sectors and disc the other plate. The deflection of the needle is proportional to the square of the difference of potential between the plates. The sensitiveness is increased by bringing the discs closer together, and also by decreasing the distance between the bifilar suspensions.

A commutator is provided for changing the above arrangement of connections, whereby the instrument is made less sensitive.

The constant is determined by employing a known difference of potential: then, $V = K\sqrt{a}$. The instrument is enclosed in a metallic case supported on three ebonite pillars terminating in three levelling screws. The aluminium discs are carried on varnished glass rods. The suspension passes through a glass tube fixed to the cover of the instrument, and is about 20 cm. long. In order that the sensitiveness should be great, it is necessary that the sectors should be as thin and as flat as possible, and that the discs should be quite parallel with one another. The following are the results of tests on an instrument built in Professor Righi's laboratory:—When connected up to give the maximum sensitiveness, and fitted with a bifilar suspension, a deflection of 51 mm. was obtained on a scale placed at a distance of 5 metres from the apparatus when working with a difference of potential of 1 volt. By using a quartz fibre suspension instead of the bifilar, the deflection was increased to about 400 mm. Under these conditions the time of oscillation was very large, and it required several minutes to take a reading.

C. GOURRÉ DE VILLEMONTÉE—ELECTRIC POTENTIAL IN A CONDUCTING LIQUID HAVING A UNIFORM MOTION.

(*Comptes Rendus, Vol.* 119, 1894, *p.* 1201.)

The liquids employed (mercury, solutions of sulphate of copper, zinc, and nickel containing 10 grammes of salt per litre of distilled water) were made to

pass through glass tubes, 3 mm. in diameter in the case of the mercury, and 8 mm. in diameter for the solutions. The platinum electrodes employed were plated with either copper, zinc, or nickel, and were so arranged that the potential difference could be measured between two electrodes in the tube itself, or between one of the electrodes in the tube and one placed in the reservoir. A very sensitive capillary electrometer was employed for measuring the differences of potential. The velocity of the liquid varied from 33·5 to 323 mm. per second. The following conclusions were arrived at from these experiments:—

1. The differences of potential observed when the liquid was at rest or when in motion were found to be identical in the case of mercury, and solutions of sulphate of copper and of sulphate of zinc. The successive periods of motion and of rest varied between 30 seconds and 6 minutes.
2. It was found that the bubbles of air in the solution produced comparatively large differences of potential between the electrodes, which were very variable during either period of motion or of rest, of the liquid.
3. It was found impossible to obtain consistent results in the case of nickel when the periods of rest or of motion exceeded 30 seconds, owing to the sudden changes which took place in the solution or electrodes.

Conclusion.—The uniform motion of a conducting liquid through glass tubes of large diameter and of equal section throughout their length, produces no appreciable alteration in the differences of potential between two points in the liquid, within the limits employed in the above case.

L. POINCARÉ—ON A CLASS OF SECONDARY BATTERIES.

(*Comptes Rendus*, *Vol.* 120, *No.* 11, *p.* 611.)

Nearly all accumulators in practical use at present depend on the oxidisation and reduction of an oxide of lead. Although much has been done to perfect the construction of lead accumulators, they still have several great disadvantages—notably, the low conductivity of the conducting parts, their great weight, their tendency to disintegrate, and the careful treatment to which they must be subjected.

In order to diminish these disadvantages, it has been suggested that liquid electrodes should be employed, and an attempt was consequently made to use mercury. Under these conditions it would be necessary to use an electrolyte consisting of a salt instead of an acid. An amalgam would then form at the cathode, which, in combination with the mercury, would constitute a secondary battery. Amongst the numerous salts on which the author carried out experiments the most interesting results were obtained with alkaline haloid salts. They are capable of forming perfectly reversible cells, having an electro-motive force of about 2 volts; but chlorine and bromine combine with the mercury at the positive pole to form a badly conducting surface, thus yielding a bad efficiency. This is, however, not the case with sodium iodide; and as long as the solution is sufficiently concentrated that the current-density is not too great, and that the surface of the positive electrode is greater than that of the negative, no deposit is formed on the

anode. It is thus possible to construct a secondary battery in which the two electrodes remain perfectly metallic after being charged, and is therefore possible to reduce the internal losses to a very small amount by suitably arranging the electrodes to offer the least resistance.

An ampere-hour or watt-hour efficiency cf 90 per cent. can be obtained with such an accumulator, and is the same for all rates of discharge. The cell in no way suffers by being short-circuited. The electro-motive force at full charge is 1·85 volts, which gradually decreases during discharge. The capacity per kilogramme is about 10 ampere-hours, being about the same as with ordinary accumulators. The electro-motive force of this cell was found to be almost independent of temperature.

A. DE BERLIOZ—THE THERAPEUTIC ACTION OF ELECTRIC CURRENTS OF HIGH FREQUENCY.

(*Comptes Rendus, No.* 11, *Vol.* 120, *p.* 644.)

During the last year the author has subjected a great number of patients to the action of high-frequency alternating currents, these currents taking place in the body by induction; and the results obtained form a clinical proof of Mr. D'Arsonval's physiological discoveries on this subject. The method consisted in placing the patient within the large solenoid designed by Mr. D'Arsonval, for periods of 15 to 20 minutes every day, care being taken to avoid all other treatment but this one.

From 20th January, 1894, to March, 1895, 75 cases were treated by the above method. The effects in some cases proved negative, but the greater number obtained great benefit from this treatment.

These currents are found to have a powerful effect on the nutritive action, and will constitute a valuable treatment for a great number of functional disorders.

DESIRÉ KORDA—A THERMO-CHEMICAL CARBON CELL.

(*Comptes Rendus, Vol.* 120, *No.* 11, *p.* 615.)

The fundamental experiment made by Becquerel (1855), and repeated by Jablochoff (1877), showed that a rod of carbon made red hot and dipped into an iron crucible containing nitrate of soda produced an electro-motive force with carbon at the negative pole. This led the author to investigate whether, during the reduction of metallic oxides by the carbon, a part of the chemical energy brought into play did not also manifest itself under the form of electrical energy. Amongst the different substances on which experiments were carried out the author found two which yielded an electro-motive force at a sufficiently high temperature, one directly with carbon, and the other indirectly—that is to say, by the interposition of alkaline carbonate in a state of fusion.

One of these substances is baryum dioxide, which in contact with the hot carbon decomposes into baryum and carbon anhydride, and, with carbon at the negative pole, produces an electro-motive force of about 1 volt.

The other substance is copper dioxide, which yielded an electro-motive force of 1 volt when separated from the carbon by a layer of carbonate of potassium.

The experiments were made with a plate of agglomerate carbon connected by a platinum wire to the negative terminal of a Richard voltmeter having a resistance of 100 ohms, and divided into tenths. A piece of baryum dioxide was connected by a platinum wire to the positive pole of the same instrument.

The carbon and the baryum dioxide were then placed in a Bunsen flame and pressed together in such a manner that the wires were not heated. At a dull red heat a strong effervescence was noticed, with production of carbonic acid. The voltmeter read from 0·85 to 1 volt. A grey spongy mass of baryum was formed on the carbon plate, with white traces of carbonate of baryum formed by the carbonic anhydride which is liberated.

The experiment was repeated by placing the two substances in a crucible heated in a coke fire. At a dull red heat the voltmeter read 0·9 volt, but this fell every time that the crucible was removed from the fire. In order to determine the internal resistance, a resistance of 4 ohms was connected in parallel with the voltmeter. The reading immediately fell from 0·8 volt to 0·2 volt. The internal resistance was then 13·6 ohms.

Similar experiments were carried out with dioxide of copper. The following are the results of one of these experiments, in which case the carbonate was quite pure and dry:—

When cold	0·0 volt.	After 45 minutes ...	1·1 volts.
After 9 minutes...	0·1 ,,	,, 54 ,, ...	1·1 ,,
,, 15 ,, ...	0·9 ,,	,, 61 ,, ...	1·0 ,,
,, 34 ,, ...	1·0 ,,	,, 75 ,, ...	0·9 ,,

In the case of the 1·1 volt reading the measured internal resistance was 3·2 ohms.

In another experiment, in which damp carbonate was employed, the voltmeter at first indicated about 1 volt in the opposite direction before giving the above results; this being evidently due to the chemical action of the steam produced.

In conclusion, the author remarks that when agglomerate carbon is employed the amount of electrical energy which accompanies the reaction represents only a small fraction of the chemical energy, which manifests itself mainly under the form of heat; but, *per contra*, in the case of graphite, although the same electromotive force is produced, the quantities of copper and of protoxide formed are in better relation to the current.

In neither case, however, does Faraday's law apply, on account of the external energy supplied under the form of heat.

L. PALMIERI—A CONTRIBUTION TO THE STUDY OF EARTH CURRENTS.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 13, *p.* 577.)

Since the discovery of the presence of earth currents by Macrini, numerous experiments have been made to establish the laws and to ascertain the cause of these phenomena. The greater number of observers employed wires laid in known

directions, but no notice was taken as to the relative level of the ends of these wires. A note was merely taken as to the orientation of the wires—according to the earth's meridian, to the magnetic meridian, and also according to a direction at right angles to the former. The current in the first conductor was called the meridional current, and the other the equatorial current.

It was usually found that the former flowed from north to south, and the latter from east to west, from which Battelli deduced mathematically that the true direction of the earth current was from north-east to south-west.

The author recalls experiments made in 1852 at the Vesuvius Observatory with reference to atmospheric electricity.

A copper wire 400 metres long was employed for this purpose. It was earthed at one end on the slope of the hill, and insulated as far as the Observatory, where it was connected up to a galvanometer; the other terminal being connected to an insulated conductor fitted with a number of points and mounted on the roof of the test room. When it was not raining no deflection was noted; but when it rained and the conductor was no longer properly insulated, a deflection was noticed, indicating the passage of a current flowing from the hill to the Observatory. The same results were obtained after altering its azimuth and making several modifications.

The same results were obtained by Mattenci at St. Maurice and in Tuscany. The same phenomenon was also noticed by Professor Ragona at Modena.

In 1889 the author obtained the use of a telegraphic line 8 kilometres long from Resina to the Observatory. The results of six years' observations have shown that when Vesuvius is not active, or during periods of minimum activity, the earth currents flow in an upward direction, irrespective of the azimuth in which the wire is placed, and that these currents increase with a diminution in the activity of the volcano: they diminish when the activity increases, and become zero. When this activity reaches a certain degree, then the currents start flowing in the opposite direction if the action of the crater increases still further. They then continue to increase proportionally with this action.

From the above results it may be stated that—

1. In the case of wires inclined to the horizon and out of volcanic effects, the earth currents flow upwards, in whatever azimuth the wires are placed.
2. In the case of wires placed in the vicinity of an active volcano, these currents diminish, become zero, and then change in direction when the volcanic activity increases. When the currents flow downwards, they increase and decrease with the activity of the volcano.

The experiments carried out since 1889 were made under the following conditions:—

From the test room, at an altitude of 637 metres, leave two wires. One is 8 kilometres long, lies in a S.W. direction, and is earthed at its further end by means of a copper plate dipping into a well. The second, which is shorter by a few 100 metres, is earthed in a valley; a copper plate being buried in vegetable soil to a depth of 3 or 4 metres. These two wires are placed in the same azimuth—N.W., S.W. A third wire leaves the test room to be connected to a local earth. This wire is called z, and the two others x and y respectively. The currents in

either x or y can be observed by suitably connecting up to the galvanometer. The instruments in use in the test room consist of an astatic galvanometer with double winding, a rheometer, of which a description has been previously given by the author, and a D'Arsonval aperiodic galvanometer.

It was found that on connecting the ends x and y to the galvanometer a very weak current was obtained, being the difference between the two ascending currents. It was noted that when the current reversed it did so in both these wires. The current in the long wire was generally stronger than in the short wire, but during the earthquakes of 1894 it was noticed that the current in the shorter wire became strongest; but in February (1895), when the shocks seem to have subsided, although the current in the short wire had decreased by one-half, it was still considerably above that in the long wire.

The following general conclusions are arrived at:—

1. In the case of wires inclined to the horizon, the earth currents flow upwards, irrespective of the azimuth, so long as the experiments are not carried out in volcanic regions, or, if so, that the volcano be in a state of complete tranquillity.
2. If the experiments be made in the neighbourhood of an active volcano, the currents will increase and decrease according to the volcanic activity, as stated above.
3. If it is desired to study earth currents irrespective of volcanic action, it would be then necessary to employ perfectly horizontal wires.

It would be necessary to note whether the direction or strength of these currents are affected by local or neighbouring earthquakes. It has been noticed on several occasions that at the period of syzygies there is an increase in the earth currents.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the month of MARCH, 1895.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHTING AND POWER.

G. RICHARD—Arc Lamps.—*Ecl. El.*, vol 1, No. 10, p. 439 (I.).

H. GEORGES—Comparative Economy of Single-Phase and Polyphase Currents.—*Ibid.*, p. 462, No. 11, p. 512 (I.).

W. STINE—On the Influence of the Carbons on the Illuminating Power of the Arc Light.—*Ecl. El.*, vol. 1, No. 12, p. 557.

F. BREISIG—On an Artificial Overhead Line.—*Ecl. El.*, vol. 1, No. 13, p. 593.

ANON.—The Utzinger Arc Lamp.—*Ibid.*, p. 599 (I.).

ANON.—Report of the Berlin Electricity Works.—*E. T. Z.*, 1895, No. 10, p. 150.

J. B. BRUNN—Details of Electric Train-Lighting.—*E. T. Z.*, 1895, No. 11, p. 163 (I.).

K. RICHTER—On Electrostatic Phenomena in Running Machinery.—*E. T. Z.*, 1895 No. 12, p. 176.

E. HOEGERSTADT—A New Method of Connections for Power Transmission.—*E. T. Z.*, 1895, No. 13, p. 185 (I.).

DYNAMO AND MOTOR DESIGN.

ANON.—Remarks on the Determination of the Behaviour of Shunt Dynamos from the Characteristic Curves.—*Ecl. El.*, vol. 1, No. 13, p. 582 (I.).

E. ARNOLD—On Unipolar Induction and Alternators with Stationary Windings.—*E. T. Z.*, 1895, No. 10, p. 136 (I.).

— DOLIVO-DOBROWOLSKY—Polyphase and Alternate-Current Dynamos of the Allgemeine Elektricitäts-Gesellschaft (Discussion).—*E. T. Z.*, 1895, No. 12, p. 181.

MAGNETISM.

— HURMUZESCU—The Electro-motive Force of Magnetisation.—*Jour. de Phys.*, March, 1895, p. 118 (I.).

M. Ascoli—On the Distribution of Induced Magnetism: Part I.—Magnetism of Hollow and Solid Cylinders.—*Ecl. El.*, vol 1, No. 10, p. 474.

J. Klemencic—On the Magnetisation of Iron and Nickel Wires by very Rapid Electric Oscillations.—*Ecl. El.*, vol. 1, No. 11, p. 522.

Anon.—On the Utilisation of the Alternating Rotary Magnetic Field.—*Ecl. El.*, vol. 1, No. 12, p. 555 (I.).

— Nagaoka—Variation in the Length of Ellipsoids of Iron, Nickel, and Cobalt under the Influence of Magnetisation.—*Ecl. El.*, vol. 1, No. 13, p. 603.

— Nagaoka—Distribution of Magnetism in a Nickel Wire under the Simultaneous Action of Longitudinal and Torsional Stresses.—*Ibid.*, p 606.

C. E. St. John—Value of the Magnetic Permeability for Rapid Electrical Oscillations.—*Phil. Mag.*, vol. 39, No. 238, p. 297.

A. Bock—On the Relation of Contraction of Area to Increase of Length in Iron Rods when Magnetised.—*W. A.*, vol. 54, No. 3, p. 442 (I.).

O. Grotrian—On the Magnetisation of Iron Cylinders: Part II.—*Ibid.*, p. 452 (I.).

W. Weiler—The Effects of Two Magnetic Fields on One Another.—*Beibl.*, vol. 19, No. 3, p. 252.

ELECTRIC TRACTION.

Anon.—Recent Experiments on Electric Traction.—*Ecl. El.*, vol. 1, No. 12, p. 551.

P. Hoho—The Employment of Two or more Motors on Electric Locomotives or Carriages.—*Ecl. El.*, vol. 1, No. 9, p. 402.

Anon.—The Electric Tramways of Philadelphia.—*E. T. Z.*, 1895, No. 12, p. 172 (I.).

L. Baumgardt—Contribution to the Question of Quick Application of Brake to Motor Cars.—*E. T. Z.*, 1895, No. 13, p. 184.

G. Kapp—Electric Railways.—*Ibid.*, p. 191.

ACCUMULATORS.

L. Poincaré—On a Class of Secondary Cells.—*C. R.*, vol. 120, No. 11, p. 611.

INSTRUMENTS AND MEASUREMENTS.

H. Abraham—Note on the Employment of the Telephone as a Zero Instrument in a Wheatstone's Bridge traversed by Alternate Currents of High Frequency. —*Jour. de Phys.*, March, 1895, p. 127.

C. J. Rolleson—Phonographic Method of Recording Alternate-Current Waves.—*Ecl. El.*, vol. 1, No. 10, p. 461.

Anon.—The Thallenberger Registering Wattmeter for Polyphase Currents.—*Ecl. El.*, vol 1, No. 11, p. 509 (I.).

Anon.—Hopkinson's Automatic Switches.—*Ibid.*, p. 515 (I.).

— Pasqualini—Measurement of Small Resistances.—*Ibid.*, p. 525 (I.).

ANON.—Siemens & Halske Exploder.—*Ecl. El.*, vol 1, No. 12, p. 552 (I.).

B. BRUNHES—On the Effect of an Alternating Electro-motive Force on the Capillary Electrometer.—*C. R.*, vol. 120, No. 11, p. 613.

R. THRELFALL—The Clark Cell when Producing a Current.—*Phil. Mag.*, vol. 39, No. 238, p. 295.

F. KOHLRAUSCH—On the Determination of Resistance of Electrolytes with Direct or Alternating Currents.—*Beibl.*, vol. 19, No. 3, p. 245.

ANON.—A New Microphone by Mercadier and Anizan.—*E. T. Z.*, 1895, No. 10, p. 145 (I.).

F. BREISIG—Researches on Induction in Cable Conductors.—*E. T. Z.*, 1895, No. 11, p. 164, No. 12, p. 174, No. 13, p. 186 (I.).

TELEGRAPHY AND TELEPHONY.

G. DE LA TOUANNE—Note on Telephony in the United States: The Law System. —*Ecl. El.*, vol. 1, No. 9, p. 411, No. 10, p. 466 (I.).

— VARTORE—The Growth of Telephonic Communication.—*Ecl. El.*, vol. 1, No 11, p. 487 (I.).

ANON.—The Submarine Cables of the World.—*Ecl. El.*, vol. 1, No. 9, p. 408.

ANON.—Telephonic Tariffs. —*Jour. Tel.*, vol. 19, No. 3, p. 49.

A. COLLETTE—Long-Distance Telephony.—*Ibid.*, p. 58.

ANON.—Utilisation of Urban Telephone Systems for Fire and other Alarms.—*Ibid* p. 61.

ANON.—Telegraphs and Telephones in Scandinavia at the End of the Year 1893.—*Ibid.*, p. 64.

ANON.—State Telephones in Austria.—*E. T. Z.*, 1895, No. 11, p. 167.

ELECTRO-CHEMISTRY.

ANON.—The Borchers Carbon Battery.—*Ecl El.*, vol. 1, No. 11, p. 507

ANON.—Thofern Electrolytic Bath.—*Ecl. El.*, vol. 1, No. 12, p. 553 (I.).

K. STRECKER—Practical Measurements made on Batteries.—*Ecl. El.*, vol. 1, No. 13, p. 597 (I.).

C. DEGUISNE—On the Existence of an Anomaly in the Conductivity of Saline Solutions at 4 Degrees.—*Ecl. El.*, vol. 1, No. 9, p. 427.

F. KOHLRAUSCH and A. HEYDWEILLER—On Alterations of Resistance of Solutions caused by Constant Electric Currents.—*W. A.*, vol. 54, No. 3, p. 385.

E. WARBURG—On Electric Conduction and Convection in Dilute and Badly Conducting Solutions.—*Ibid.*, p. 396 (I.).

A. J. WAKEMAN—On the Influence of Small Quantities of Electrolytic Substances on the Molecular Conductivity of Acetic Acid.—*Beibl.*, vol. 19, No 3, p. 246.

R. BERNOUILLI—On the Influence of the Solvent on the Electro-motive Force.—*Ibid.*, p. 248.

R. LÜPKE—An Attempt to Review the more Recent Theories of Electrolysis.—*Ibid.*, p. 249.

G. SCHMITZ—Experiments with a Carbon Iron Cell.—*E. T. Z.*, 1895, No. 10, p. 145.

— FRANK—The Manufacture of Acetyl, and its Use in the Production of Illuminating Gas, Alcohol, &c.—*Ibid.*, p. 146 (I.).

— HÄUSSERMANN—Electricity in the Service of the Chemical Industry.—*Ibid.*, p. 153.

STATIC AND ATMOSPHERIC ELECTRICITY.

H. O. ELLINGER—Index of Refraction of Alcohol for Electric Radiation.—*Jour. de Phys.*, March, 1895, p. 133.

H. EBERT and E. WIEDEMANN—On Electric Discharges, Production of Electric Oscillations, and their Part in the Phenomena observed in Discharge Tubes.—*Ibid.*, p. 133.

A. LE ROYER and P. VAN BERCHEM—Measurement of the Length of Wave of a Hertzian Primary in Air by means of the Change of Resistance of Metallic Particles.—*Ibid*, p. 142.

NILS STRINDBERG—On Multiple Resonance of Electric Waves.—*Ibid.*, p. 142.

A. RIGHI—On Electric Oscillations of Short Wave-Length, and their Employment in the Production of Phenomena Analogous to the principal Phenomena of Optics.—*Ecl. El.*, vol. 1, No. 9, p. 391, No. 10, p. 448, No. 11, p. 495, No. 12, p. 541, No. 13, p. 583 (S. I.).

K. MACK—Double Refraction of Electric Radiation.—*Ibid.*, p. 472.

G. W. PIERCE—On Resistance to Discharge.—*Ecl. El.*, vol. 1, No. 12, p. 558.

L. PALMIERI—New Contribution to the Theory of Earth Currents, derived from Observations made at the Vesuvius Observatory with Wires inclined to the Horizon and arranged in different Degrees of Azimuth.—*Ecl. El.*, vol. 1, No. 13, p 577 (I.).

C. A. MEBIUS—On the Brush Discharge in Air.—*W. A.*, vol. 54, No. 3, p. 520 (I.).

W. TRABERT—On the Theory of the Electric Phenomena of our Atmosphere.—*Beibl.*, vol. 19, No. 3, p. 260.

THEORY.

M LAMOTTE—Propagation of Varying Currents in Conductors.—*Ecl. El.*, vol. 1, No. 10, p. 433.

H. PELLAT—Electrostatics not founded on Coulomb's Laws: The Electric Force acting at the Surface of Separation of Two Dielectrics.—*Ecl. El.*, vol. 1, No. 11, p. 481.

J. CAURO—Approximate Calculation of Capacity Effects in Bobbins.—*Ecl. El.*, vol. 1, No. 12, p. 529.

— CLAVENAD—Definition of Mass: Capacity for Movement. An Absolute System of Physical and Mechanical Quantities, applicable to all Phenomena.—*Ibid.*, p. 536.

J. ANDRADE—On the Potential of an Electrified Surface.—*C. R.*, vol. 120, No 11, p. 605.

J. Cremer—A Contribution to the Elementary Theory of the Idea of Potential in Electricity: Part I.—Electrostatics.—*Beibl.*, vol. 19, No. 3, p. 245.

H. A. Lorentz—Attempt at a Theory of the Electrical and Optical Phenomenon in Bodies in Movement.—*Ibid.*, p. 259.

VARIOUS.

— Wind—On some Recent Experiments relating to the Kerr Phenomenon.—*Ecl. El.*, vol. 1, No. 12, p. 565.

Anon.—Static Electricity in Printing Establishments.—*Ecl. El.*, vol. 1, No. 13, p 595 (I.).

A. Blondel—The Determination of the Mean Spherical Intensity of Sources of Light.—*Ecl. El.*, vol. 1, No. 9, p. 385 (I.).

G. Vincentini and M. Cinelli—Transmission of Electricity through Gas surrounding a Conductor made Red Hot by an Electric Current.—*Ecl. El.*, vol. 1, No. 9, p. 422 (I.).

D. Korda—A Carbon Thermo-chemical Battery.—*C. R.*, vol. 120, No. 11, p. 615.

A. Griffiths—Some Experiments with Alternating Currents.—*Phil. Mag.*, vol. 39, No. 238, p. 229.

E. T. Jones—On Electro-magnetic Stress.—*Ibid.*, p. 254.

K. Strecker—On Electrical Conductivity.—*W. A.*, vol. 54, No. 3, p. 434.

Threlfall, Brearley, and Allen.—Researches on the Electrical Properties of Pure Substances: I.—The Electrical Properties of Pure Sulphur.—*Beibl.*, vol. 19, No. 3, p. 246.

P. Duhem—The Electro-dynamic and Electro-magnetic Phenomena: Part II.—Electro-magnetic Phenomena.—*Ibid.*, p. 252.

S. Stricker—On Current Electricity.—*Ibid.*, p. 262.

Anon.—The Electrical Standardising Laboratory of the English Board of Trade.—*E. T. Z.*, 1895, No. 10, p. 140 (I.).

K. Moritz—A New Safety Device for Use with Overhead Conductors.—*E. T. Z.*, 1895, No. 12, p. 176 (I.).

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXIV. 1895. No. 118.

The Two Hundred and Seventy-eighth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, April 25th, 1895—Sir DAVID SALOMONS, Bart., Vice-President, in the Chair.

The minutes of the Ordinary General Meeting held on April 4th 1895, were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Students to that of Associates:—

Charles Herbert Archer.	J. J. F. O'Shaughnessy.
John George Burchell.	George F. Pilditch.
E. E. Gunter.	Phillip James Watts.

Mr. F. V. Andersen and Mr. H. E. Mitchell were appointed scrutineers of the ballot.

The following paper was then read:—

A MAGNETIC TESTER FOR MEASURING HYSTERESIS IN SHEET IRON.

By Professor Ewing, F.R.S., Member.

Professor Ewing.

Makers of transformers are now generally alive to the paramount importance, from the point of view of all-day efficiency, of using iron in which the hysteresis losses are small. The need, however, is felt of some simple means by which this quality can be readily determined. Tests made by the ballistic and other methods have shown that different samples of iron may exhibit extraordinarily wide differences in regard to hysteresis. The author has found, for example, in the course of his own tests of iron supplied for transformers, a range of nearly three to one in the hysteresis of good and bad samples. It is unnecessary to point out how much the all-day efficiency of a transformer working under ordinary conditions will be affected if the sheet iron used in its construction has three times as much hysteresis as it need have. Again, experience shows that even in one batch of plates, of the same rolling, so much difference is apt to exist between one plate and another as to make the testing of a single sample furnish a poor guide to the average quality of the batch. Even in a single plate samples cut from different parts show marked differences in hysteresis. The author has noticed as much as 15 per cent. variation in samples taken from the four corners of a plate not quite 2 feet square, the material of which was of very excellent quality. It is well known that different analyses of the nearly pure iron of which such plates are rolled exhibit corresponding variations. The impurities appear to be unequally distributed; and, further, it is not unlikely that the irregularities which are found in the magnetic quality depend in part on slight differences in the treatment as regards annealing. Whatever their cause, there can be no doubt that they exist, and their existence makes the result of a single test often somewhat fallacious. What is wanted is a sufficiently large

Professor Ewing.

number of tests, in any one batch of iron, to allow a fair average to be estimated for the quality of the batch.

To make this possible a simple and expeditious method of testing is imperative. The ballistic method, although unimpeachable on the score of accuracy, is anything but expeditious, and can scarcely be called simple. The value of the hysteresis is arrived at by first observing the relation of the induction, B, to the magnetising force, H, for a considerable number of points in each of a series of cyclic processes of magnetisation, then drawing the B H curve for each of these cycles and measuring the enclosed area. The operation requires some skill and experience, and a good deal of patience. The same criticism applies to other methods of finding the hysteresis by first finding the relation of magnetism to magnetising force (as, for instance, by the permeameter or by the author's magnetic curve tracer). It must be borne in mind that in the testing of transformer iron the permeability is a quite secondary matter. What is of primary importance is the hysteresis, and high permeability is not necessarily or invariably found associated with small hysteresis. In the testing of sheet iron by the ballistic method the finding of the relation of B to H is a step in the process; it is not the object of the test.

These considerations have led the author to devise an apparatus which measures the hysteresis directly, by a single operation, and in a manner sufficiently simple to allow the test to be applied to numerous specimens without much expenditure of time, trouble, or iron. The basis of the instrument is the mechanical measurement of the work done in causing reversals of magnetism to take place in the iron under examination. Such measurements have been made before as a laboratory experiment and under special conditions. Thus, Mr. L. R. Wilberforce and the author, two or three years ago, endeavoured to determine the hysteresis of a cylinder of iron when rotated in a strong magnetic field, by measuring the couple required to keep it slowly turning. And a method of the same general character has been employed with complete success by Mr. F. G. Baily to investigate the disappearance of hysteresis which takes place in such a cylinder when the

Professor Ewing.

field is made sufficiently strong—a matter of great interest in connection with the molecular theory of magnetism. In the machine now brought forward as a practical tester for workshop use the same mechanical principle is involved. The process of reversal, however, resembles that which occurs in a transformer rather than in a dynamo; the induction is caused to have practically the same value in all samples, and that a value appropriate from the transformer point of view; the sample is cut in a very simple form, and is arranged to be readily inserted and removed. The magnetism is reversed by turning a handle, and the result is given by the position of a pointer on a scale.

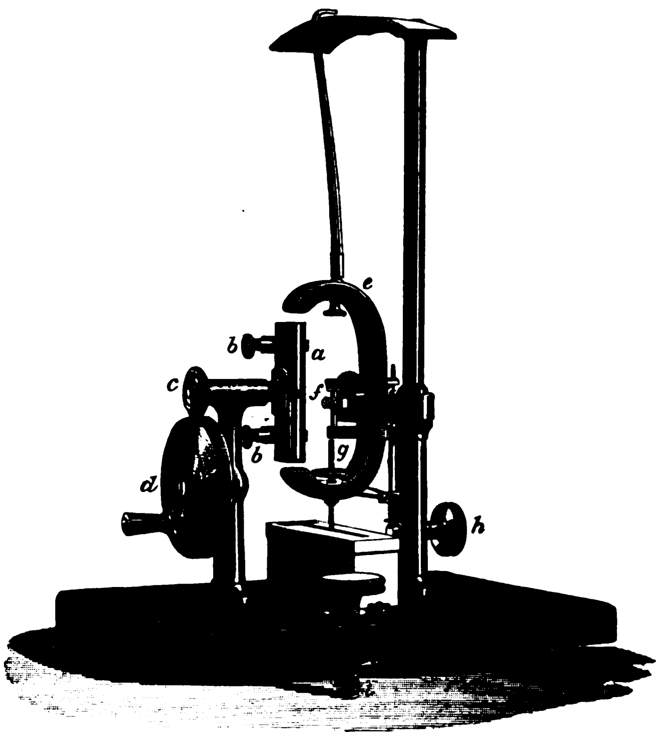


FIG. 1.

Fig. 1 is from a photograph of the instrument now exhibited, the form of which has been reached after a good deal of experiment. The iron to be tested is cut or stamped in the form of

Professor Ewing

strips, which are 3 inches long and five-eighths of an inch wide. The number of these pieces that is taken to compose the sample depends on the thickness of the sheet: six or seven pieces will in general be sufficient for the usual gauges of transformer iron, and a smaller number if the material tested is the thicker sheet used in dynamo armatures. The bundle of pieces forming the sample is placed in a carrier, *a*, and is covered with a vulcanite washer and secured by two clamps, *b b*. The carrier is made to rotate by means of the friction pulley, *c*, and driving wheel, *d*. This causes the sample to revolve between the poles of the permanent magnet, *e*, with the effect that its magnetism is periodically reversed. The work done in reversing the magnetism, in consequence of hysteresis, causes a mechanical moment to be exerted by the revolving sample upon the magnet; and the magnet, being supported on a knife-edge at *f* in line with the axis of the carrier, tends to follow the sample and is deflected through an angle which serves to measure the work expended. Since a definite amount of work is done per reversal, whatever the frequency (so long as that is not so high as to make a sensible addition to the work by inducing currents), the deflection of the field-magnet is independent of the speed at which the carrier revolves, and no special care has to be taken to turn the handle at a uniform rate. If the rate is very slow the magnet will show each individual impulse which it receives as the ends of the sample pass its poles, but when the speed is sufficiently raised these impulses blend into a steady deflection; and the speed may be further augmented, to the extent of doubling it, or more, without making the deflection change. It is only at higher speeds still that the effects of induced currents become apparent. The deflection is observed by means of a pointer and scale above the magnet. The swinging of the magnet is checked by means of a dash-pot below, consisting of a vane, or spade, moving in a box filled with oil. The stability is adjusted to give any required degree of sensitiveness, by means of the weight, *g*, which travels as a nut upon a screw fixed to the magnet, and serves to raise or lower the centre of gravity of the oscillating system. The magnet swings about a knife-edge

Professor Ewing.

working in an agate trough, and a lifting arrangement like that of a balance is provided, operated by the handle, *h*, to save the knife-edge from unnecessary wear or injury. The pointer is set to zero in the middle of the scale by means of a nut which runs on a screw projecting sideways from the middle of the magnet, and a more delicate adjustment of the zero may be effected by means of the levelling screw, *i*. In operating, the observer inserts the sample and secures it by the clamps, then begins to turn the handle and lets the magnet down on its knife-edge. After reading the deflection of the pointer to one side he reverses the direction of rotation, reads the deflection to the other side, and takes the sum of the two readings as the total deflection. The deflection is proportional, or very approximately proportional, to the hysteresis of the iron, even when samples widely different in quality are compared.

To secure that this shall be so, a considerable air space is left between the magnet poles and the ends of the sample, with the result that such variations of permeability as are liable to be met with in different samples are almost without influence upon the total induction through the iron. The author has examined the induction by means of a search coil wound round the sample, and has found it to be practically the same in the best and worst specimens of transformer iron. The dimensions and strength of the field-magnet are so proportioned, with reference to the section of the sample and to the extent of the air gaps, as to make the induction have a value fairly representative of transformer work. In the instrument shown the induction is about 4,000 C.G.S. units with the normal size of test sample. By increasing or reducing the area of section of the sample the intensity of the induction is reduced or increased. Within reasonable limits, however, any value of this intensity may be adopted in testing, for it is known from the results of ordinary ballistic tests that curves drawn to show the relation of the hysteresis loss to B preserve their relative positions through a wide range of values of B. In other words, the relative amounts of hysteresis in different samples do not change, or scarcely change, when the iron is more strongly or less strongly mag-

netised. This, of course, follows from the fact that a formula of the type originally suggested by Mr. Steinmetz, Professor Ewing.

$$\text{Hysteresis loss} = c\,B^n,$$

is practically applicable, c being constant for any one iron, and n a general constant, within limits which are more than sufficiently wide to include the inductions used in transformers.

In cutting the pieces which are to make up the sample care has to be taken to make the length always the same, otherwise the air gaps, and consequently the induction, will vary irregularly. The carrier is formed so that it serves on a length gauge; and to facilitate the preparation of the samples a supplementary gauge is furnished, in the form of a clamp of hardened steel, in which the strips are readily filed to the exact length after being cut a trifle too long. The width of the pieces requires no particular care.

The author was under the expectation that it would be necessary, in making an accurate comparison of quality between different specimens, to give the sample the same weight in each case, in order to make it have the same cross section. He finds, however, that no nice adjustment of the weight is required, provided advantage is taken of a property which will be most easily explained by citing an experiment made with the tester.

The experiment in question shows the effect of varying the section of the iron by varying the number of strips which make up the sample. Eleven strips of thin transformer iron were taken, each 3 inches long by five-eighths of an inch wide, cut from the same plate. These were all used to form the sample in the first instance; then one was removed, and the remaining 10 were tested, then nine, and so on, until finally the sample under test consisted of a single strip. The following are the scale readings for this, and also for another exactly similar experiment, which was made with a different specimen of transformer iron, of a poorer quality than the first. The two specimens are distinguished as A and B :—

Professor Ewing.

Number of Strips in Sample.	Scale Readings of Magnetic Tester.	
	(Specimen A.)	(Specimen B.)
11	72	118
10	71	117½
9	70	117
8	69½	116½
7	69	116½
6	69	117
5	69	120
4	71	124
3	75	132
2	85	142
1	69	130

These results are also shown in the curves of Fig. 2. It will be

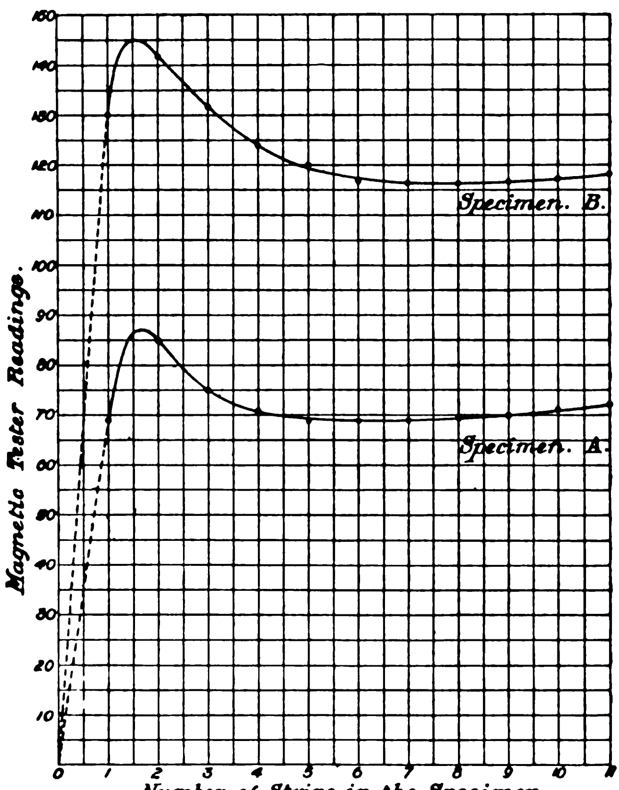


Fig. 2.

Professor Ewing.

seen that both curves have the same general character, and that the scale readings of the tester pass a very flat minimum when about seven pieces form the sample. Whether six, seven, or eight pieces be used, there is no material difference in the readings. Precisely similar results are given by other specimens. Hence, if a weight is chosen equal to that of seven pieces of this material as a general standard, it will suffice, in preparing other samples for test, to select that number of strips which will give a weight approximating even roughly to the standard weight. The nearest whole number of strips will do, and it is not necessary to cut a narrow or wide strip to make up the exact weight.

When only two or three strips of this thin iron are taken the work expended in reversing the magnetism of the bundle is increased. The total induction through the sample is somewhat diminished; but the intensity is increased, and this more than compensates for the smaller volume of iron that is being operated on. It is only when the sample is reduced to a single piece that the saturation of the iron prevents B from increasing sufficiently to continue this effect, and there is then a sharp fall in the curve.

These considerations have led to the use of samples the aggregate thickness of which is equal to that of six or seven thin transformer plates. In testing armature plates the same weight is approximated to by taking a smaller number of pieces. It is scarcely necessary to add that solid pieces, or bundles of wire, may be tested as well as sheet iron.

Calibration.—An extensive series of experiments has been made in order to determine to what degree of accuracy the readings of this magnetic tester correspond to the hysteresis losses as found by ballistic tests for various samples of iron. For this purpose it was necessary to test each identical sample in as nearly as might be the same state, first ballistically, and then in the new tester. In view of the variations which are observed between one part of a plate and another, the author did not feel that it would be satisfactory to use separate pieces for the ballistic observations and for the observations to be made with the tester. After some consideration he adopted the following two plans:—

Professor Ewing

In the first place, the ballistic tests were made on rings, each consisting of a long strip cut to the width subsequently wanted in the magnetic tester, namely, five-eighths of an inch. This was bent into a ring, the ends just overlapping. After bending, the ring was annealed in a gas flame to get rid of the hardness produced by the bending. This is a most essential step, for in good iron the process of coiling into a ring, even of large diameter, hardens the metal very perceptibly and increases the hysteresis. Then the ring was tested ballistically, a sufficient number of cycles being taken to allow a curve to be drawn showing the relation of the hysteresis loss, or $\int H\,d\,I$, to B, from B = 2,000 to B = 8,000 or so. The same thing was done with each ring. Then the rings were uncoiled and cut up into 3-inch lengths for the tester, and the pieces were straightened and annealed to get rid of the hardness due to this straining. The whole number of pieces into which each ring had been cut were then dealt out into packs of six (or five, or seven, according to the thickness of the material), and these packs were shuffled until repeated trials in the tester showed that every pack gave nearly the same scale reading. The mean scale reading for the several packs obtained from each ring was then taken as a mean applicable to that ring. Here it was assumed that the first and second annealings might be held to bring the material into the same physical state.

To guard against any uncertainty on this point, four additional trials were made, using a different procedure. Long strips were again cut, but each of them was tested ballistically in its original straight form in a solenoid with open ends, the induction coil being wound over the central region only. Each strip was 36 inches long and five-eighths of an inch wide, and a single strip was used in each test. The general effect of the free ends is to shear over the B H diagram, but not to alter the enclosed area. Enough areas were measured to determine, as before, the curve of $\int H\,d\,I$ and B for each strip. The strips were then cut up into pieces for the tester, which were treated as in the former case. In this procedure there was no annealing.

By these two means seven standard samples in all were produced, of quality ranging from exceptionally good to ex-

ceptionally bad. The magnetic tester readings for these were taken, using approximately equal weights to form the sample in each case. In the following table the tester readings are given alongside of the value of the hysteresis loss ($\int H\,dI$) as determined ballistically for an induction, B, of 4,000 units for each of the seven specimens. Numbers I., II., and III. were those tested ballistically in the ring form; the others were tested in the form of straight strips. Professor Ewing.

Comparison of Magnetic Tester Readings with the Hysteresis Loss determined by Ballistic Tests.

No. of Specimen.	Magnetic Tester Scale Readings.	Hysteresis Loss by Ballistic Tests. Ergs per c.cm.
I.	60	640
II.	81	890
III.	124½	1,420
IV.	70	750
V.	125½	1,430
VI.	136	1,580
VII.	184	2,120

The same results are shown graphically in Fig. 3. It will be seen that they accord well with one another, and that the scale reading of the magnetic tester is nearly proportional to the hysteresis loss. A straight line drawn from the origin among the observed points lies sufficiently near all of them to allow the assumption to be made, so far as workshop testing is concerned, that the deflection in the tester, when multiplied by a factor depending on the sensitiveness of the instrument, is a practical measure of the hysteresis.

More exactly, however, the locus of the points is seen to be a straight line passing not quite through the origin. This may be partly due to the fact that the induction produced in the tester is not absolutely so high in the bad as in the good iron. In all probability, however, it is mainly due to this—that as the

Professor Ewing.

sample revolves some small amount of work is done in periodically changing the distribution of magnetism in the field-magnet

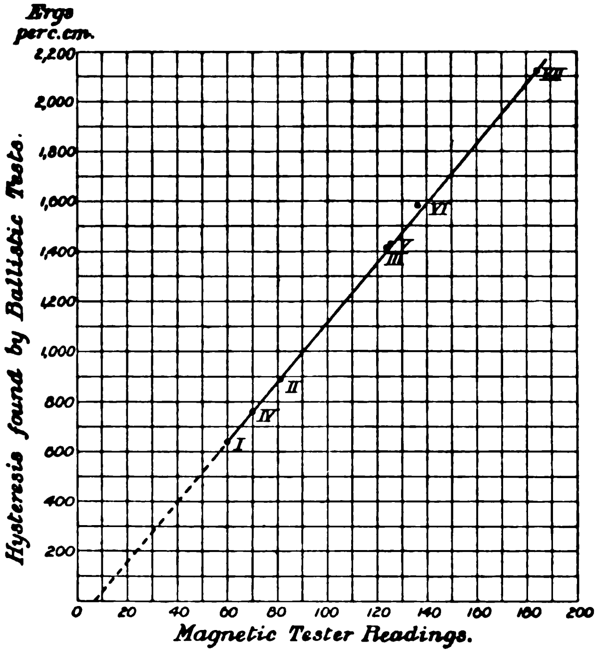


FIG. 3.

itself, in addition to the work that is done in reversing the magnetism of the sample. Any work expended within the field-magnet would contribute to the deflection, and would exist even if the sample (while retaining its permeability) were entirely destitute of hysteresis. Hence the line may be expected to pass a little to the right of the origin, as it is actually found to do.

It is intended to furnish with each instrument two standard samples of iron, along with a table stating their hysteresis as found by reference to ballistic experiments (in ergs per cubic centimetre, and also in watts per lb., at an assigned frequency). In proceeding to test other samples the operator will first note the deflection given by each of his standard pieces, and then, by simple proportion, or by aid of a diagram, he may go on to interpret in absolute measure the deflection observed with any specimen. This will make his results independent of any loss of

strength on the part of the field-magnet, or of any changes he may find it convenient to make in the sensibility of the instrument. Professor Ewing.

The hysteresis of the standard samples is stated for B = 4,000. To find the hysteresis for any other induction it is only necessary to use the following table of factors, which has been deduced by the author as a general mean from ballistic tests of many samples:—

Induction, B.		Relative Amount of Hysteresis.
2,000	...	0·33
2,500	...	0·47
3,000	...	0·63
4,000	...	1·00
5,000	...	1·41
6,000	...	1·89
7,000	...	2·41
8,000	...	3·00

Other forms of the instrument can be constructed by making the field revolve while the sample is pivoted, so that the sample deflects; by placing the axis vertical instead of horizontal, with a control furnished by a spring or by bifilar suspension; by using a weighted arm or a torsion head to measure the deflecting couple, instead of observing the deflection by a pointer on a scale; and so forth. But the author believes that the form now exhibited will be found both accurate and handy in general use.*

He hopes it may contribute indirectly to the betterment of the iron used for magnetic purposes, by making it easy for a purchaser of iron or a purchaser of transformers to specify a maximum of hysteresis, and to see that he gets what he specifies. A notable improvement in the quality of sheet iron in this respect has been going on during the last few years, and it is now practicable to get iron which a short time ago would have been

* In one of his early experimental machines the author used discs—or, rather, rings—of iron. But he concluded that strips were much to be preferred, partly because they took less iron, and partly because they made the process of reversal resemble more closely the process in a transformer, but, above all, because they could be readily prepared by anybody without special tools.

Professor Ewing.

pronounced impossibly good. But it would be easy to quote tests showing that some iron of relatively very poor quality still finds its way into armature and transformer cores. The author's aim has been to put into the hands of those who are practically concerned with magnetic hysteresis, whether as makers of plates, builders of transformers, or engineers of alternate-current supply stations, an apparatus in which testing is reduced to its simplest terms, where each test consumes no more than a few square inches of the sheet, where the preparation and testing of the sample take only a few minutes of time, and the operation consists in turning a handle and reading how good or bad the iron is from the position of a pointer on a scale.

Sir D. Salomons

The CHAIRMAN: Gentlemen,—We have a duty to perform before we enter upon the discussion of this valuable paper. I think the first thing is to congratulate Professor Ewing on the fact of his having recovered from his recent illness. You have just heard a paper which would have been most welcome to us three or four years ago, but it is better late than never. I am sure such an instrument as the one he has brought before us will not only prove valuable to those who work in the laboratory, but that its value will be great to the manufacturer and the engineer. There is another class also that will be greatly benefited by the results of Professor Ewing's work in this direction, and that is the consumer of electrical energy. All inventions which tend to improve apparatus for the production of electrical energy will cheapen its cost to the public, and will be welcomed equally by the electrical industry and engineers. I have seen a masterly report showing the great importance of the quality of iron used in transformers. If the shareholders in electric lighting companies get bread sometimes when the iron is good, they will get their meat and dessert when it is perfect. I do not wish to forestall any remarks that may be made to-night by borrowing from the work so well done by a gentleman who is present this evening. The gentleman to whom I refer is Professor Fleming, and I will call upon him, after we have given a vote of thanks to the author of this paper, to commence the discussion, if he will so kindly oblige us.

A vote of thanks was unanimously accorded to Professor Ewing for his interesting paper.

Professor Fleming.

Professor FLEMING: It is impossible not to admire the ingenious solution of a practical problem which Professor Ewing has brought before us. I think it possesses three qualifications for a workshop method. In the first place, it is independent of steadiness; it works with small samples; and, in the next place, the process occupies a very short time. These are certainly great recommendations from the point of view of workshop use. We know perfectly well that in going through the process of evolution an instrument like this passes through a number of stages before it arrives at the point at which the inventor brings it before the public, and no doubt Professor Ewing has made many different forms of the instrument before he reached this one. One would like to ask him why he prefers to revolve the specimen, and not the magnet. No doubt he has probably tried that. At first sight, it seems the simplest thing to do to revolve the magnet, and to have specimens consisting of discs of iron suspended by a torsion wire, or some such arrangement, which would then be deflected. That might have some advantages, perhaps, but Professor Ewing will give us reasons against it. One thing that might be probably more easy to test with that form would be the effect of temperature upon hysteresis. It must not be forgotten that the hysteresis loss in iron varies very considerably with different temperatures. In transformer tests I have found the difference in the hysteresis loss between the transformer taken quite cold and when taken at about 100° C., when it has reached the temperature it would have after being connected some time with the circuits and on open secondary circuits, is as much as 10, 12, or 15 per cent.; and that difference is certainly an important difference. Then, again, although it is not a suggestion for an improvement, it is an obvious modification to make the field revolve in different ways. If any one possesses a two- or three-phase alternator, it would be possible to make a revolving field which could travel round the specimen. One would like to be assured, in using the instrument, that the brass clamps which hold the

Professor Fleming.

specimens together do not have eddy-currents set up in them. That, I have no doubt, Professor Ewing proves by putting a dummy into the holder and revolving it thus, proving that there is no sensible deflection due to any eddy-currents set up in the brass clamps. It would, of course, be an important application of this instrument to assist to settle that question which has exercised the minds of many of us recently, viz., whether there is a time increase in the hysteresis loss in iron by use. That has been discussed a good deal in the pages of technical journals, and many experiments have been made on it by myself, Professor Ewing, and others. It is perfectly certain it does not occur in all transformers. It seems also that it does occur under some circumstances; and it is of little use to dispute about minute differences in the core losses of transformers, when under some circumstances there may be an increase of 30, 40, 50, or more per cent in the total loss of the transformer in time. If iron ages magnetically—although not exactly in the same sense as port wine ages, in the way of improvement, but by way of deterioration—then certainly it is not much use to wrangle about very small differences indeed, when very large ones may be brought about by use. These, of course, are important practical questions, as we all know, and any instrument which facilitates the solution of them is certainly an additional gain in our instrumental means. No one who has used the ballistic method, even in laboratories, is very much in love with it, and the conditions under which we have often to do these tests would render it impossible. Professor Ewing has deserved our thanks by adding one more means of attacking these problems in magnetism.

Professor Foster.

Professor CAREY FOSTER: I should like to echo what my colleague, Professor Fleming, has said as to the beauty of this instrument Professor Ewing has brought before us. But I should like to ask, if I may, one or two questions about it. At first sight it occurs to one that it will be possible to deflect a magnet like that, so delicately suspended as that is, by causing a bit of wood, or anything else, to rotate between the poles. Anything rotating between the poles would act as a fan and tend to blow the magnet round. No doubt Professor Ewing knows all about that,

Professor Foster.

and how to make allowance for it, if any such effect does occur to an appreciable extent. Again, there is another point Professor Fleming mentioned—that time lag, or magnetic viscosity, would act on such an instrument in the same way as true hysteresis. It is not obvious how this instrument would distinguish between the two effects. This, again, is a point that cannot have escaped Professor Ewing, and I mention it to ask him to add to the information he has given us by showing how he takes account of it.

Professor Ayrton.

Professor AYRTON: There is one thing which charms me about this instrument, especially as it has been constructed by a Cambridge Professor, and that is, the observer has simply to turn a handle; the instrument possesses, as you see, a slot, and if you have the necessary penny with which to purchase the apparatus, the result is achieved! Certainly, if all one wants to do in testing iron is to ascertain approximately the hysteresis loss, no better instrument than the one that is on the table could be used. If, however, one wants to know the hysteresis loss for different inductions, is it quite right, as Professor Ewing has said, to assume that the ratio of the hysteresis losses of different specimens of iron with different inductions is the same? The author has rather implied in the paper that it is so; but, if he remembers, in his own paper—published with Miss Klassen—he gives the hysteresis loss for various irons at various inductions; and from those results you can calculate, as Mr. Mather was so kind to calculate for me to-day, a table from which you will find that irons which may be said all to have the same hysteresis loss at an induction of 4,000, varied by 8 per cent. in the hysteresis losses at an induction, of 8,000, and by 12 per cent. at 2,000. That would mean that if you wanted the hysteresis loss very accurately for different inductions it would be necessary to experiment with those different inductions for each specimen of iron. Of course it may be answered that when you are dealing with irons where the hysteresis losses vary by hundreds per cent., 12 per cent. is of no consequence whatever, and therefore it may be good enough to test the various specimens with one induction, and to assume that at any other induction the hysteresis loss in

Professor Ayrton.

the various specimens will bear the same proportion as they have at the induction used in the test.

There is another point which must not be forgotten, and that is, that for the designer of a dynamo, or for the designer of a transformer, the hysteresis of the iron is not all that has to be known. It may seem a roundabout way to measure the permeability if you only want to know the hysteresis; but if you want to know the permeability as well—and I do not see how you could very well make calculations regarding the amount of iron to be used unless you did know the permeability—it is not a roundabout way to get the hysteresis loss by first measuring the permeability.

Then comes the question, Can you measure the permeability of iron easily, or must you employ the time-honoured ballistic galvanometer? In the paper, the author implies that you cannot get the hysteresis loss quickly by first finding the relation of magnetism to the magnetising force. That is not quite my experience. Some two years ago, I wanted to see whether it was not possible to make an instrument which would enable you to get the B H curve for any specimen of iron quite easily. I may mention that my object in designing the instrument (a very simple one, as you will see in a moment) was to make something that would be acceptable to mechanical engineering students, some of whom work in my laboratory. Mechanical engineering students are interested in dynamos, motors, and transformers, but they do not care particularly about the vagaries of a ballistic galvanometer. It therefore seemed to me that the best plan was to make a little separately excited dynamo, and to use the specimen of iron you desired to test as the yoke of the dynamo. The instrument was constructed under the superintendence of Mr. Mather, and has been frequently used by my students in getting B H curves of a specimen of iron. There is a little motor driving a little separately excited dynamo at a uniform speed, the dynamo having very massive pole-pieces, and a very narrow air gap for the size of the machine. The tops of the pole-pieces are joined by a magnetising coil into which the rod, or bundle of plates, of iron to be tested, is inserted and

clamped in position, the test bar, or plates, constituting the yoke of the dynamo. In circuit with this coil are accumulators, an adjustable carbon resistance, an ammeter, and a reversing key. The armature, which is rotated by a little motor at a constant speed, is of the Pacinotti type, and attached to the brushes is a voltmeter. There you have the whole apparatus. The ammeter reads off the magnetising current, and the voltmeter the magnetic induction; and the apparatus has the great advantage that the induction is read off by a perfectly steady deflection, and not by the instantaneous swing of the needle of a ballistic galvanometer. Professor Ayrton.

The carbon resistance can be varied so as to cause the magnetising current to have a number of different values, plus or minus, fairly quickly, and so a number of points on the B H curve between any desired plus and minus limits of magnetising force can be obtained in a few minutes; indeed, a whole series of complete B H curves can thus be experimentally obtained in the course of half an hour or so.

Of course this apparatus has the disadvantage, compared with the beautiful instrument Professor Ewing has brought before us, that, if you only want to ascertain the hysteresis loss, you must measure the area of the B H curve; but if you want to know both the permeability and the hysteresis, then you can get the information fairly quickly with the motor dynamo device which I have described. But I end by repeating what I started with—that if you desire to make fairly good approximate experiments on hysteresis loss only, then Professor Ewing's apparatus is certainly the simplest instrument yet constructed.

Professor GEORGE FORBES: There are one or two remarks I should like to make. I begin by joining in the thanks offered to Professor Ewing. It certainly is a great thing to have produced such a beautifully simple apparatus. I think there is no doubt that in transformer work, hysteresis is the great thing that we have to look after. The permeability is good enough in a piece of iron that has little hysteresis. The instrument before us commends itself to us most particularly because it is really a thorough working one. It is a practical instrument for testing samples Professor Forbes.

Professor Forbes.

that are being used in manufacture. I should say it is essentially not a laboratory instrument, while it is admirably adapted for the measurements that we wish to make in the shops. It is not, I think Professor Ewing would admit, the most refined instrument that could be obtained for laboratory work: he would doubtless, for work of that nature, much prefer the methods for obtaining hysteresis which he has already so well brought out in previous years. In fact, there are some points about the instrument which to me are a little unintelligible, and perhaps they are so to others; and I trust that, by drawing attention to them, Professor Ewing may enlighten us a little more on them. I am a little puzzled about that 4,000 intensity of induction. It is difficult for me to see that it ought to be 4,000 induction in each case, whether we have four strips of iron or eight strips of iron.

Professor EWING: It is not so at all. It has only 4,000 induction when there are about seven strips. In that particular iron, if you diminish the number of strips you increase the intensity of the induction and you slightly diminish the total induction, but the total amount of work consumed in hysteresis remains nearly constant.

Professor FORBES: My notion was simply that the action that takes place at the edge of the magnetic lines of force must obviously produce considerable disturbance when you vary the number of strips, but that has been thoroughly taken care of by Professor Ewing. Any such criticism of the instrument as a purely scientific instrument is utterly valueless, when we consider for what purpose the instrument is chiefly intended, viz., for giving us practical tests with a close approximation; the straightness of the lines in Diagram 2 are sufficient for all practical purposes. I do think, however, that Professor Ayrton has hit upon a point of importance as to the density—that is, the strength of the induction in the samples of iron under consideration; that it is very important to have different densities, different magnetic inductions, and that we cannot depend upon the hysteresis being proportional in different specimens of iron at different densities. That may be a little source of error; but I have no doubt Professor Ewing holds that the variations are not sufficient

Professor Forbes.

to be of any importance in comparison with the enormous variations that we have to consider between different qualities of iron. So also, for the same reason, I presume, Professor Ewing considers it quite immaterial whether the strength of the field magnet diminishes with age as long as it does not become really bad: as long as it does not vary more than 50 per cent. or so, I suppose he would consider the result would be fairly the same; because we must remember we are always comparing a sample with a standard sample, and that is our check upon the calibration of the instrument. I think Dr. Fleming drew attention to the possibility of eddy-currents in the brass work, and I would suggest that the eddy-currents in the iron itself may possibly also be quite perceptible. If they are, however, they would probably appear immediately upon considerably increasing the speed of the instrument; and if you can vary the speed very largely without altering the readings so as to give very erroneous results, I think that that source of error is probably of no importance. I do think, however, that I would prefer that explanation of eddy-currents in the iron, rather than the explanation which Professor Ewing gave as the reason why the straight line in Fig. 3, which covers all the observations, does not pass through the zero. Obviously, if the iron were perfectly without hysteresis, there would be a certain amount of work done owing to eddy-currents at the zero point, and that would not be shown by the ballistic method, and would account for the deviation of the straight line from the zero point. These are really the only observations which have occurred to me in regard to this apparatus. It seems to me a perfectly beautiful instrument, and one we have all been waiting for for a very long time. When I heard that Professor Ewing was getting out such an instrument, before I knew the method on which it was founded, I recalled the apparatus which Professor Elihu Thomson made some years ago, and which I believe is being made by the General Electric Company, although I have never seen the instrument. He cuts his sheet iron into circles, builds them up into a small cylinder, and he mounts them upon a pivot, retaining them in position by a spiral spring. He then rotates—as Professor Fleming

Professor Forbes.

suggested—a field magnet round the cylinder, and he measures the deflection produced in the cylinder by the torsion due to hysteresis, the restraining force being the force of the spring, and the deflection being proportional to the force required to overcome it. The apparatus is somewhat the same as Professor Ewing's, but, on considering the relative merits of the two, I think in all probability this is simpler and better. At any rate, I am sure that we are all much indebted to Professor Ewing for bringing this beautiful instrument before us.

Mr. Siemens.

Mr. Alexander Siemens: Three years ago I brought some results about hysteresis before the Institution, and all the measurements mentioned in the paper were made by the ballistic method. Although, I daresay, we shall obtain some of these instruments, I think, on the whole, we shall adhere to the ballistic method. Professor Ewing has himself said, in the beginning of his paper, that specimens cut from the same plate may vary considerably, and he says that treatment of the specimen is of great importance. That is precisely what we have found. If you wish to determine the hysteresis of any specimen, you must not handle it in any way. You dare hardly look at it without its changing its hysteresis altogether! Professor Ewing says that when he bends his strips into a ring, in calibrating his instrument, he has to anneal the ring to restore the previous state of the iron; but, from experiments we have made, we do not think you can restore the previous state of the iron by annealing. We have had a very sad experience, and perhaps it may serve as a warning to others. In the paper which I brought before you I spoke very hopefully of cable transformers —that is, of transformers which were to be made on a cable machine—where a rope of iron was to be made and spun round with the primary and secondary conductor. We found that it was simply impossible to make these cable transformers sufficiently good as regards hysteresis; because, however good the iron was which we took, however good the individual wires were which we received from the manufacturers, the mere fact of laying them up in a strand, the mere manipulation of them, destroyed them magnetically, if I may say so, and the hysteresis increased to

such an extent that these cable transformers were no use whatever. I speak so much on this point because I want to draw attention to what I consider the only drawback in this instrument, namely, the filing of the specimens. I am perfectly certain that if a millimetre more or less is filed off it will at once make the results of the readings quite different, owing to the mechanical disturbance which the specimen has undergone. As I said at the commencement, where very accurate scientific results are required the ballistic method is the safest. Professor Ewing has reproached that method with being slow and not easily performed. Of course, if you get the man in the street to take readings on a ballistic galvanometer, you will not succeed; but if you take people who have been educated as electrical engineers, who have passed through an electrical testing room, and know how to read a galvanometer, they can make these tests very quickly. I may say that with the apparatus as arranged in our works a reading of a specimen can be completed in half an hour—that is, about the same time that Professor Ayrton mentioned that his instrument would take to get a complete curve which gives you the full information as to permeability and hysteresis. Mr Siemens.

Professor AYRTON: I said half an hour for a *series* of curves. One single curve may be got in about three or four minutes.

Mr. SIEMENS: I mean to say you get the complete information in half an hour. I certainly think, however much this treatment of the samples may affect them, the instrument is a remarkably handy one, and will certainly be extensively used in our profession.

Mr. S. Z. DE FERRANTI: The beauty of the instrument which Professor Ewing has shown us to-night, and its perfect adaptability for the purposes for which it is most generally required—namely, testing transformer iron—so much impressed itself upon my mind that I was immediately seized with a desire to know where it was made, where it was on sale, and how much it could be bought for. But this idea was rather dispelled when Professor Ayrton showed us another instrument. I immediately wondered whether it would not be necessary to have that one as well, in order to know what one was doing with transformer iron. I think, however, from the remarks made by the various speakers, that the instrument which Mr. Ferranti.

Mr. Ferranti.

Professor Ewing has shown to us to-night, coupled with an accurate means of reproducing samples without a varying class of treatment, would serve the purpose most perfectly for everything one could want in the way of transformer work. I would suggest that it would be more accurate to properly stamp a sample than it would be to cut it, which involves a certain amount of bending and filing up. If one were fitted up for daily testing—that is to say, testing which would be no trouble whatever, and which could be done as a most everyday sort of thing by unscientific people—if one was provided with such an instrument as Professor Ewing's, and a punch which one could put the sheets into and simply blank out a sample, anneal it for so many minutes in a muffle, and then put it in the instrument, I think the uniformity of samples tested would be such that one would have accurate knowledge of the iron one was using, and be able to keep a check on the various kinds of iron used for making transformers.

Mr. Esson.

Mr. W. B. Esson: I should like to endorse all the nice things which have been said about the instrument. If it does all that Professor Ewing says it does, it is something we have been looking for for a long time. I think it is only a manufacturer that can appreciate at its true value an instrument like this, because the manufacturer is heavily fined on account of excess watts lost in the iron of his transformers, and anything which will enable him to escape being fined is to be welcomed. It is very true, as Professor Ewing would admit, that this apparatus does not give tests having the same scientific accuracy as the ballistic method; yet I can conceive no instrument better adapted for making rapid pass tests of iron in bulk, such as are required in the factory. Its chief advantage lies in its extreme simplicity: anyone can use it; it might be safely entrusted to the foreman of the shop.

Mr. Baily.

Mr. F. G. Baily: I should like to say a word or two, as Professor Ewing has mentioned my name in the paper, and I have been trying some experiments with a rotating magnet for a somewhat more scientific purpose than that intended by Professor Ewing; and it is with considerable disappointment that I find he has applied it so satisfactorily for a useful purpose, whereas I did

Mr. Baily.

not. I thought the only purpose served in using a rotating magnet with a rotating field was to test armature iron, and I did not consider that testing iron of armatures for hysteresis was of sufficient importance to bring out a commercial instrument, as it does not much matter, to a small amount, what the hysteresis in armatures is. The method employed is certainly the right principle for measurement of hysteresis, and I congratulate Professor Ewing on his ingenuity and success in applying it to an alternating field. There are, however, one or two points concerning which I should like to ask him. From his paper, before examining his diagrams, I did not think the instrument would work as well as it seems to do. First of all, as regards Fig. 2, and the alleged immaterial thickness and size of the plates, it is quite easy to see how the induction in all kinds of wrought iron stays constant when the samples are of the same size; but I cannot quite see how the total hysteresis in samples of different sizes stays constant. If the area of the sample is increased, and the induction density remains the same, the total amount of induction will be increased. Therefore, as there will be the same amount of hysteresis per unit volume, the result will be an increased pull on the magnet. If, however, the total number of lines of force remains the same, the induction density will be diminished, and, as the hysteresis diminishes more rapidly than the induction density, the pull will be diminished. I suppose that both these effects occur, and, acting in opposite directions, approximately neutralise each other; but it seems dangerous to assume that the balance will always be exact. That is the only explanation I can make for the flatness of that curve, and it is borne out by the shape of the other parts of the curve.

I should also like to ask Professor Ewing whether he has considered the effect of demagnetisation while the sample is at right angles to the poles of the magnet. When it is away from the poles, before it gets to the other pole, it must demagnetise itself to a certain extent, as it is only a short piece of iron; and therefore the repulsion, when it gets to the opposite pole, will be less than it would be were no demagnetisation to occur. Of

Mr. Baily. course, if the demagnetisation were the same in all specimens, that would not matter, because the effect would be included in the constant of the instrument; but it is not the same in all specimens. It depends upon the retentiveness of the iron. I think that is a possible explanation of the variation of the line in Fig. 3. Professor Ewing points out that in good iron the readings low down on the line were somewhat greater when taken on the tester than on the ballistic galvanometer—that is, that the value of the hysteresis given by the instrument is too great. I suggest to him that one possible explanation of that variation is, that the iron represented by the lower points has not demagnetised itself so much. Good iron demagnetises itself less than bad iron, and therefore the upper points give really somewhat too low a value for hysteresis, and they should be further along; that is to say, the values given on the rotating instrument are too small. There are doubtless several other small items which make up both these sets of curves, and some of them act in one way and some in another. The eddy-currents will certainly cause some error, but it is rather difficult to say how much. I was rather surprised to hear that Professor Elihu Thomson had employed a rotating field, as I thought Professor Ewing and myself were the only ones who used it for that purpose. Apparently Professor Thomson employed almost exactly the same kind of instrument as I had myself.

[*Added.*]—Another possible source of error lies in the fact that at the ends of the sample the direction of the molecules is not symmetrically reversed, but there is also a certain amount of rotational action. That the values of the hysteresis in these two cases are identical it is unwise to assert, as I know of no comparative tests of the two values carried out on the same piece of iron. My own tests seem to indicate a distinct difference between them, the rotating field causing the greater hysteresis at low inductions; but the point certainly requires further demonstration, and should not be assumed without proof.

Mr. Trotter. Mr. TROTTER: I should just like to ask one practical question. I do not quite understand whether the specimens are annealed after cutting to size. The specimens seem to me so much smaller

than is usual in test pieces, and so very much smaller than those used in transformers, that the hardening of the edges due to shearing or punching might be appreciable, and it seems doubtful even if annealing would get rid of the distress which the edges must suffer from shearing or punching. Mr. Trotter.

Mr. L. J. STEELE: I am sure that a hysteresis meter so ideal in its simplicity as the one described to-night will be welcomed by those who have frequently to measure the losses in iron, by methods which have hitherto been comparatively elaborate, and which necessitated a considerable amount of time and trouble. Mr. Steele.

It seems to me, however, that there would be some difficulty in accurately predetermining the iron loss in a transformer with this instrument, owing to the fact that no measure is made of the loss due to eddy-currents—a loss which increases with the thickness of the iron sheets, and which varies as the square of the induction and of the frequency.

It would be interesting to compare the losses in the iron of a transformer with those obtained in the tester, thus affording an accurate method of separating the two losses, and of investigating the effects due to different thicknesses of iron, &c.

The following is a comparison which I have made between the relative amounts of hysteresis at different inductions, obtained from tests on a transformer working at 100 cycles per second, and those given in the table of factors:—

B.	Relative Amount of Hysteresis.	Relative Amount of Iron Loss from Test on Transformer.	Percentage Difference.
4,000	1	1·02	2·2
5,000	1·41	1·49	5·5
6,000	1·89	2·03	7·5
7,000	2·41	2·62	8·5

As is seen, there is a difference between the two sets of factors, which would lead to considerable errors at the higher inductions. The differences between the two are partly due to the rapid increase of the eddy-currents.

For accurate transformer designing it would therefore be

Mr. Steele. necessary to somewhat modify the table of factors given in the table.

With reference to the increase in hysteresis in iron produced by bending, I have found that in some cases this effect is considerable. In one experiment, a strip of annealed charcoal iron was bent up to form a rectangular frame, the bends at the corners being rather sharp. Under these conditions, the iron loss was found to be very high. This iron was carefully annealed and re-tested. The losses were reduced by 45 per cent.

It has occurred to me that the principle adopted in this tester, of reading the deflection of a balanced magnet, is susceptible of a further application, and might be advantageously employed for measuring the eddy-current losses in armature conductors by rotating sample conductors between the poles of a suitable magnet.

Professor Ewing. Professor EWING, in reply, said: I must, in the first place, thank all the members who have spoken for the very kind expressions they have used with regard to this new instrument and its probable practical applications. I will take the points raised in the discussion in order, as far as possible. Professor Fleming asked why I had selected a form in which the specimen revolved rather than the magnet. The point he raises is a very important one, and was, in fact, carefully considered by me in the early experimental stages of the instrument. At one time I had almost definitely made up my mind to use the revolving magnet and the fixed specimen, and it was only when some further considerations came out that I began to see that the revolving specimen and the fixed magnet were much to be preferred. If the specimen were made to be the stable body which undergoes deflection through the rotation of the field magnet, then, of course, it would be necessary that the specimen should always have precisely the same amount of mechanical stability. Now, in putting various specimens into the holder, or carrier, there would be not a little practical difficulty to ensure that there should always be the same amount of mechanical stability. It is much better that the thing which requires to have a definite and not very large amount of mechanical stability

should be a thing which has not to be handled, and the parts of which have not to be varied. That was the principal reason, but there were several minor considerations, which need not be gone into now; and when I found the instrument could work out to a practical and convenient form in this way, I was satisfied to let well alone. Then another point was raised by Professor Fleming, and also, I think, by Professor Carey Foster and Professor Forbes, and perhaps by other speakers, viz., the influence of eddy-currents and fan action on the deflection of the magnet. As regards the fan action, one can easily enough see that there is no important mechanical influence of that kind by simply turning the carrier round without any specimen in it, or putting in, as one of the speakers suggested, a dummy specimen made of wood or vulcanite. Although the magnet has not a very large amount of stability, it is not so delicately poised as perhaps some of the speakers supposed. If the dash-pot were removed it would swing with not a very slow period, and there is enough stability to prevent the fan action being noticeable. As regards eddy-currents, it was mentioned in the paper that when the speed of the revolving sample is increased, even to the extent of about doubling it beyond the speed necessary to get a steady reading, you do not find any appreciable increase in the amount of the deflection; and to my mind this is quite a sufficient proof of the unimportance of the eddy-currents, seeing that their effect would increase in proportion to the square of the speed. If you turn the sample quite slowly, you see the individual impulses which the magnet receives with every passage of the sample. Turn it a little faster, and these impulses blend into a continuous deflection. Turn it faster still, and you will fail to see any change in the steady deflection of the pointer. There is no material effect—in fact, no appreciable effect—of eddy-currents within the limits of speed that are used in the instrument; and this, of course, applies both to the eddy-currents in the brass work and to the eddy-currents in the iron itself. I should add, however, that the brass work has been so made as to reduce the eddy-currents in it as far as possible: the parts are light; there is a vulcanite washer placed on both sides

Professor Ewing.

Professor Ewing.

of the sample, so that the brass does not come into contact with the iron, and the clamps are of such a shape that they do not encircle the iron. Professor Fleming spoke of laboratory applications of the instrument, and I am glad he did so, because several other speakers seemed rather to be under the impression that this is a workshop instrument and nothing else. Of course it is primarily a workshop instrument, but during the last two months I have found it a very interesting laboratory instrument as well, and I think it will be found so by others. After having had a good deal of experience in the way of testing by the ballistic method, and now also of testing by this new method, I must confess a most distinct preference for the new method over the ballistic method for all except a comparatively small number of purposes. There are many points which can be inquired into with much greater facility where the process of testing is short and simple, as it is here, than where it is so elaborate and tedious as it necessarily is when the ballistic method is employed. If you want, for instance, to take a large sheet of iron and investigate the extent to which different parts of it differ from one another in respect of hysteresis, you can cut or stamp strips from various parts of it, or cut it all up into strips if you like, and test the whole set of bundles of strips in a few minutes by means of this instrument. To carry out the same examination by means of ballistic tests would be a work of several days, when you come to consider, not merely the taking of the observations, but the reduction of them. It was only the other day that the engineer of an alternate-current supply station sent me a slip of iron which he had taken out of a transformer, and asked me in a friendly way to see if it was good iron. It was not the case of a fee, and I was therefore distinctly unwilling to apply the ballistic method to it. Those who have had reports from me of tests made by the ballistic method will quite appreciate this point. Now in a few minutes I had cut this iron up into a number of strips, put them in the machine, and tested them. I found a large amount of hysteresis—quite double the amount that there should be in good soft iron. This was iron taken out of an actual transformer which had been in use for

several years. I then had the curiosity to anneal this same specimen, and the effect of annealing was to remove just 49 per cent. of the hysteresis, leaving 51 per cent. of it. Whether this meant that that particular specimen of iron had been put into the transformer unannealed to begin with—which I think was very probably the case—or whether it meant that the use of the transformer during these years had had the effect of deteriorating the quality of the iron to that extraordinary extent, I am, of course, unable definitely to say. There are many questions which should be quickly and easily answered by means of an instrument of this sort which could scarcely be answered by other means, except at the cost of a very much larger expenditure of time and trouble. I look forward to making experiments of a laboratory character with this tester on such questions as those which have recently been engaging the attention of Mr. Mordey and others—viz., the influence of prolonged heating upon iron, and that other question which Mr. Siemens mentioned to-night—and a very important and interesting question it is—whether successive annealings of iron always do bring the iron into the same physical condition, or not, as regards hysteresis. I think, with him, it is quite an open question; but my own experience so far shows that so long as the annealing is done in the same way—viz., by holding the specimen in a gas flame for a few minutes, until it gets red hot, and then simply taking it out and letting it cool in the air—so long as the process of annealing is repeated in the same way, it does seem substantially to produce the same result each time; but I can well believe that annealing in a closed furnace and keeping the iron hot for a long time, may produce a different effect as regards the hysteresis from that which is produced by annealing in a gas flame with quick subsequent cooling. Professor Ayrton is right in saying that the ratio of the hysteresis at various inductions is not absolutely the same in all specimens of iron. He quoted some figures, which I am unable at this moment to verify, from a paper (of which I was joint author) published in the *Philosophical Transactions* some years

Professor Ewing.

Professor Ewing.

ago. He spoke of such differences as 8 per cent.; but I think that must have been between extreme cases, and not between the general mean and any individual case. If he had taken the general mean and compared it with individual cases, he would have found the differences to be much smaller. But the general mean table of factors which I gave to-night is the embodiment of a much larger number of tests than those which are mentioned in the paper referred to by Professor Ayrton; they include many recent professional tests of transformer iron, and it is particularly to transformer iron they refer, and not to other sorts of iron and steel, such as were dealt with in that paper. I think it will be found that the ratio of the induction in various specimens of transformer iron does not exhibit very large differences at different inductions; and if we take such an induction as 4,000 as a generally convenient mean, applicable to much transformer work, we may fairly pass to inductions somewhat higher or somewhat lower than that by means of the table of factors which I have quoted. Again, Professor Ayrton objected to testing simply for hysteresis, on the ground that the hysteresis was not all that was wanted. Now, as regards transformer work, I venture to say that it is the hysteresis that is wanted, and practically nothing else. It is true there is no very definite relation between hysteresis and permeability, beyond the relation which was pointed out by one of the speakers to-night, viz., that if you get a sample with a very little hysteresis you may safely take its permeability for granted. That, I believe, is perfectly true, and fairly enough expresses the rational attitude of the transformer builder in regard to tests of sheet iron. Professor Ayrton described a very interesting instrument of his own and Mr. Mather's invention which might very possibly facilitate the testing of iron if one wished to find the B H curve, and to take its area, and then to determine the hysteresis from that area in a manner similar to that which is done in applying the ballistic method. He said, "You get the B.H. curve and the thing is done." I say, in answer to that, that the thing is not done at all. My experience is, you get the data for the B H curve after a very respectable

amount of solid work, and then you have to take the results home and spend a good many hours in reducing them, in drawing the curves, in measuring the areas by the planimeter, and, finally, in reducing the measurements of the areas, before you are able to conclude what your hysteresis is; and I confess that that process does not commend itself to me, after a good deal of experience of it, if the information that is really wanted can be got by turning a handle. Professor Ewing.

I was much interested in Professor Forbes's notice of Professor Elihu Thomson's instrument, which seems to be of a very similar character to the experimental apparatus used with such remarkable success by Mr. Baily in establishing his discovery regarding the disappearance of hysteresis in a revolving field at high inductions.

Mr. Siemens raised an important point in saying that you can hardly dare to look at a specimen without altering the hysteresis. It is perfectly true, with, of course, the humorous exaggeration that he intended. It is most difficult to deal with really soft iron in any way without producing some small effect, and sometimes a large effect, upon its hysteresis; and I think it is very probable that, as Mr. Ferranti indicated, anyone who wishes to use this instrument on a considerable scale, making many tests with it, will find it most convenient to punch his specimens to the exact size he wants, and to treat them in precisely the same way as his transformer stampings are themselves treated. But Mr. Siemens, if I did not misunderstand him, seemed to over-estimate the difficulty of preparing the samples. Of the samples which you see on the table, many were not annealed after being cut from the plate, and they were not materially deformed in the process of cutting. The iron is so thin that it is not difficult to cut it without seriously bending it, and the subsequent trimming of the ends with the file can scarcely have any hardening effect. The amount to be filed off need be no more than a small fraction of a millimetre; and by using the clamp, which holds the iron strips between two thick pieces of hardened steel, no skill is required to file the specimen

Professor Ewing.

to the exact length. The pieces of steel are made so hard that the file will not touch them.

Mr. Ferranti asked me whether the instrument can be got. That is a delicate question to answer before a scientific society, and I have felt bound to ask the Chairman whether it was legitimate even to hint at a reply. Up to now the instrument was not to be got anywhere; it has not been put on the market before to-night, so that this Institution might have the bloom of the peach, if one may use so highly figurative an expression. But from to-morrow, if Mr. Ferranti will consult Messrs. Elliott Brothers, he will no doubt get the fullest possible answer to the question, which it would be improper for me to reply to more particularly.

Mr. Trotter asked whether I annealed the specimens. That has been more or less answered in the reply to Mr. Siemens. The samples which are to be tested should, as far as possible, be tested in the same state as the stampings that they are intended to represent, and should be subjected to the same treatment; and I would suggest that, if a transformer builder is using one of these instruments, in ordering his transformer stampings he may find it convenient to order at the same time sufficient quantities of the small stampings required for the purpose of testing, and have them annealed in precisely the same way as his transformer stampings are themselves annealed.

I think most of the questions have now been answered, and I have only once more to repeat that my very hearty thanks are due to the speakers, and to the meeting at large, for the particularly kind reception which you have given to this invention.

The CHAIRMAN: I now have to announce that the scrutineers report the following candidates to have been duly elected:—

Member:

Francis George Hart.

Associates:

Joseph J. Atkinson.
Henry Brazil.
Arthur Edward Hadley.
William Henry Nimmo.
Henley Phillips.
Sydney Louis Rhys Price.

Student:

Harry Hughes Harrison.

The meeting then adjourned.

The Two Hundred and Seventy-ninth Ordinary General Meeting was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 9th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on April 25th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

D. F. Adamson.	A. S. Baxendale.

Harold W. Kolle.

From the class of Students to that of Associates—

Henry O. Fleetwood.	George Kemp.
Sydney S. Galsworthy.	Frederick Bennett Pell.

Mr. E. Talbot and Mr. A. T. Snell were appointed scrutineers of the ballot for new members.

The PRESIDENT: I have great regret in announcing the death of Major-General R. H. Stotherd, C.B., R.E., Member. He was one of the original members of this Institution when it was the Society of Telegraph Engineers; and he served on the first Council. He was intimately connected with the earliest developments of the electric telegraph for military purposes, and was equally well known in connection with the use of torpedoes for the purpose of coast defence. He was for five years connected with the Ordnance Survey of Great Britain and Ireland, during the last three and a half years of which period he was Director-

General. He contributed several papers of much interest to the Society. As he has been so closely connected with the Society since its formation, I am sure I shall only be anticipating your wishes in moving the following resolution:—"That the President, "Council, and members of the Institution of Electrical Engineers "desire to record the deep regret occasioned to them by the "death of Major-General R. H. Stotherd, C.B., R.E., one of its "earliest members; and they further desire to express to Mrs. "Stotherd and the other members of his family their sympathy "with them on their bereavement."

Captain H. RIALL SANKEY: I beg leave to second that motion; and I feel it a privilege, although a sad one, to be able to do so, because I am much indebted personally to the late General Stotherd. Amongst other things, it was through him that I became a member of this Institution. General Stotherd obtained his commission in the Royal Engineers in 1847, and filled many important posts during his service. He was always held in high esteem by his brother officers; and those who, like myself, served under him will gratefully remember his many acts of kindness, coupled with his hearty, genial manner. It will no doubt be of interest to the members to hear that from 1866 to 1871 he was instructor in electricity at the School of Military Engineering, Chatham; and that from 1873 to 1876 he was specially employed at the War Office as a member of a committee on torpedoes. As we have already heard from our President, he was Director-General of the Ordnance Survey for the last three and a half years of his active service, and during this time he took great interest in electrical matters. By his initiative, improvements were made in the method of depositing copper electrically for copying the engraved copper plates of the ordnance maps, both at Southampton and at Dublin; the arc light was introduced for photography; and the new buildings were fitted with electric light. I beg to second this motion.

The motion was unanimously agreed to.

The following paper was then read :—

ON THE RECENT DEVELOPMENT OF THE SINGLE-ACTING HIGH-SPEED ENGINE FOR CENTRAL-STATION WORK.

By MARK ROBINSON, Member.

Mr. Robinson.

1. The success of the high-speed engine in this country is amongst the notable engineering facts of the time. It relates in the first place to mechanical engineering, yet it also concerns this Institution, since it is electrical engineers who are the chief users of high-speed engines; to them, in fact, it is due that such engines hold their present important position.

2. The Presidential Address at the commencement of the present Session, in which the high-speed engine was favourably referred to, produced the comment in a leading professional paper, that electricians had little cause to boast about their work in developing high-speed engines, for if they "had stuck to the "builders of mill engines and marine engines, instead of trying to "strike out a new line of steam-engines for themselves, electric "lighting would have been very much the gainer." The judgment of electrical engineers has thus been arraigned before the general body of their brethren, and it may be useful to inquire with what justice.

3. According to what purports to be a full list of public lighting stations, published last January in one of the principal electrical journals, the indicated horse-power installed in all the lighting stations in Great Britain was 101,390. Of this, one make of single-acting high-speed engine accounts for 53,340 horse-power, or more than all other patterns put together, and even of these last, more than 4,000 I.H.P. are high-speed engines of other kinds. Engines of the mill or marine pattern figure for little in the total, and as the high-speed engine started later than its rivals, its proportion of the more recent work is even greater than the above figures would indicate. Not much apology, therefore, is needed for bringing this subject before electrical engineers.

4. The preponderance of one type of high-speed engine, embodying many views in conflict with those upon which other engine practice is based, virtually obliges this paper to be both a description and a defence, and may even give to it the appearance of eulogy, of that particular engine. Few but those directly interested in the manufacture of a highly-specialised machine are so conversant with it and its history as to be willing to write upon it; hence such a paper risks the charge of partisanship, and is certainly open to that of prepossession. A well-remembered charm of the late Mr. P. W. Willans's papers upon steam-engine economy lay in the fact that his conclusions were as far as possible framed to apply to engines in general; his own engine, though in all cases the one tested, was scarcely described, and certainly unpraised. The present paper, on the other hand, if it is to have any practical value, must not only describe the Willans engine, but must deal almost exclusively with its difficulties, with its successes, and with its improvements. A description of the engine is given in an Appendix. Mr. Robinson.

5. The advantages of single action and of high speed are, however, not limited to any one engine, and they may be described generally. An engine is "single-acting" when the steam acts only upon one side of the piston, the typical case being James Watt's Cornish pumping engine, and this method of using the steam has certain economic advantages in which a double-acting engine cannot share. At equal speeds, however, they are neutralised by the fact that, for a given power, the cylinder capacity has to be twice as great in a single- as in a double-acting engine, and it is not until the single-acting engine runs at twice as many revolutions as its rival that it has much advantage. This it is usually able to do, since it can be worked upon the "constant-thrust" plan, which the double-acting engine cannot be; that is to say, the piston-rod can be kept pressing against the connecting-rod, and the latter against the crank-pin, not only throughout the working stroke, but through the return stroke as well; the working parts are thus always in compression, and *never* in tension. The cross-head pin presses always against the same side of the bush or brass in the small end of the connecting-rod;

Mr. Robinson.

the big-end upper brass maintains constant pressure against the crank-pin, never leaving it even momentarily; the crank-shaft presses always against the lower main bearing brasses, never against the caps.* Of course a single-acting engine is not necessarily a "constant-thrust" engine, and special means have to be taken to make it so: the point is that these means are possible with a single-acting and are not possible with a double-acting engine; and though it is a view which is contested, it is the opinion of the author, and certainly it is a widely-held opinion, that really high speed and "constant thrust" should go together, unless in such a case as is referred to in paragraph 8.

6. High speed is at last recognised as a good thing in itself. A fast-running engine is, for a given power, smaller and lighter than a slow one, while a large number of small impulses per unit of time give a more even turning moment than a small number each of greater magnitude. By shorter exposure of the cylinder surfaces to successive changes of temperature, the loss by initial condensation is reduced, so that an unjacketed engine becomes as good as a jacketed one; and for many purposes, as where direct coupling to fast-running machines is required, and especially for dynamo driving, the high-speed engine has advantages which are of necessity admitted. Attempts to revive the belt-driving system in central stations, in the country which is the home of direct driving, and after the rest of the world has almost unanimously come round to its views, will be endured in England only so long as shareholders and ratepayers remain ignorant of the waste of money it entails; while the proposal to make the naturally fast-running dynamo into a slow and enormously more costly machine, merely for the sake of allowing it to be driven direct by a slow-running engine, is not likely to meet with any greater success in the long run. The future, however, can take care of itself; it is enough that high speed is already recognised as beneficial by all who are likely to mould future practice.

7. It is evident that a single-acting engine in which there

* A single-acting engine can be run with the parts always in tension, and never in compression, with equal ease, but the contrary plan is practically the best.

is absolutely no back-lash and no reversal of stresses, and which is otherwise rightly proportioned, will run freer from wear in its brasses than a double-acting engine, at any but very moderate speeds. In the author's opinion an ordinary double-acting engine is in danger of becoming troublesome, if it is run much more than one-third as fast as the speed at which a single-acting engine will run continuously and easily; will need very good design and careful attention if it is to run half as fast; and must be an altogether exceptional engine if it is to run two-thirds as fast, without giving comparatively frequent trouble. There would probably be no difficulty in finding in use many single-acting engines of, say, 80 or 100 I.H.P., running at 450 revolutions per minute, which have had five years' service and upwards without one of the brasses being touched since the engine was first put to work; and it is a safe statement that if these brasses were now removed, in most of them no wear would be detected without careful measurement, while if, through unfavourable conditions of lubrication or otherwise, appreciable wear were found, it would be absolutely without effect upon the good running of the engine. No such statement could be made of any double-acting engine; but for any purpose where the runs are limited to hours, or even to days, an ordinary double-acting engine of the same power would no doubt run at 150 revolutions without giving appreciable trouble. Such an engine, however, would have larger cylinder volume than the 450-revolution single-acting engine, and would suffer more from initial condensation, and it would hardly be trusted by most engineers for purposes (such as electrolytic work) in which an engine must run for many weeks, or even months, without stop. With special design, half-speed, or 225 revolutions (giving equal cylinder volumes, but double exposure of the surfaces in each revolution), is also attainable by a double-acting engine; but few are likely to dispute the opinion that two-thirds speed—*i.e.*, 300 revolutions per minute—is not a desirable speed for an 80-horse double-acting engine required to run continuously for weeks or months together under commercial conditions. The author does not overlook the fact that double-acting engines are designed and

Mr Robinson.

Mr. Robinson.

made to run at even the fullest speeds of single-acting engines, and that by a combination of excellent design and workmanship with a system of forced lubrication (oil is forced into the bearings by pumps) they are able to do so. It would be out of place to refer here in detail to the author's reasons for disbelief in the ultimate acceptance of this ingenious and bold attempt to evade the natural difficulties which the case presents; it is sufficient to say that for the present the single-acting engine holds the field for high-speed work on the large scale, and, according to the most recent experience, with increasing success. Defining high speed as the highest speed which experience shows can be attained without visible disadvantage by a single-acting engine of a given power, it must be said that single action is not essential to high speed, but that it helps enormously to its successful attainment.

8. It may nevertheless be of interest to remark (though, in the author's opinion, the fact has no bearing upon electric lighting practice) that for the attainment of the extremest speeds, with large powers, a double-acting engine is to be preferred. The maintenance of constant thrust upon the brasses, which may be taken as the real essence of a single-acting engine, involves great difficulties when certain speed limits are exceeded, and if, through any temporary reason, the constant thrust is lost and the engine becomes double-acting as to its brasses, there will be serious chances of rapid wear, and even of such hammering as to cause fracture. In a double-acting engine, on the contrary, provision is made from the beginning for the change of stress at the end of each stroke; it is a mere question of brute strength in the parts, and since they are always under observation for excessive wear, and periodical adjustments are a part of the system, there is little danger of serious damage. For torpedo boats and other cases where the greatest power must be obtained from the least weight, and where knock and frequent adjustment are comparatively unimportant, double-acting engines are plainly best. But few electrical engineers would care to entrust the continuous running of their stations to engines of this type, or to go through quite the same engine-room experiences as are rightly accepted on board a "catcher."

9. The class of engine called "high-speed" in America is usually a double-acting engine, capable of running from one-half to two-thirds the speed of the corresponding single-acting engine of this country. As such it has already been referred to generally, and though it usually runs well, it has no claim to special consideration, certainly not on the ground of economy. Mr. Robinson.

10. The effect of "single action" in lessening wear upon the brasses must be obvious, and the connection is almost as close between single action and the beautifully simple method of automatic lubrication by the splash of the cranks in an oil chamber; the latter cannot be adopted without much inconvenience, where brasses have to be under constant observation. The very small bearing friction, which results partly from this system of lubrication, and partly from the absence of back-lash, is also unattainable in a double-acting engine.

11. So far only the general and more obvious advantages of the single-acting engine have been considered. Now it is proposed to describe in some detail three important features in the particular engine which has become, in this country at least, the type of the class; none of them have yet attracted as much attention as perhaps they deserve. They are—

(*a*) The special system of packing rings for pistons, valves, and glands.

(*b*) The special provision for internal drainage.

(*c*) The Cornish cycle, or "transfer plan."

All three are directly related to the single-acting principle. The first two are peculiar to the Willans engine, and none of them could possibly be adopted in a double-acting engine, except the special packing rings, and those only by completely duplicating them upon each piston. All are of high value in promoting steam economy.

12. **PACKING RINGS** (*a*).—One of Mr. Willans's latest improvements in his engine was the system of packing rings shown in Fig. 1, where A is the working end of the cylinder; the pressure in B, below the piston, is materially less upon the down (or working) stroke than that above it, but on the up or exhaust stroke it is practically the same: it is never less, unless in the

Mr. Robinson.

exceptional case of a looped diagram. There are two rings, *a* and *b*, breaking joint, and turned of the same thickness throughout,

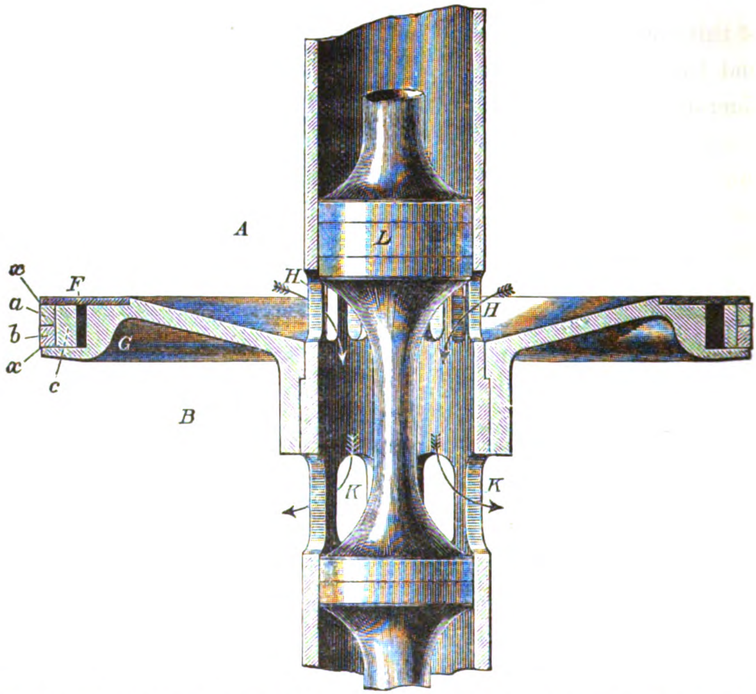


FIG. 1.—Vertical section through piston, showing rings **a** and **b**, which break joint; spring **c** for *lightly* forcing the rings outwards; spring follower plate **F**, with holes through it admitting steam to the space behind the spring **c**.

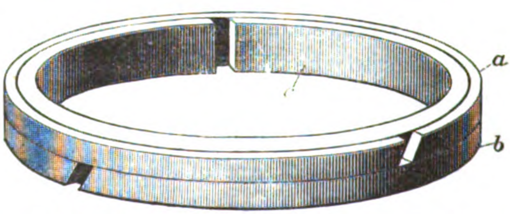


FIG. 2.—Perspective view of set of piston rings complete. Two rings, **a** and **b**, breaking joint, and spring **c**. The steam presses against the inner surface of **c**. NOTE.—The thickness of the rings is exaggerated relatively to their diameter, for greater clearness.

which exactly fit the cylinder before they are divided; *c* is a cast-iron spring ring, turned eccentric and slightly too large for the cylinder: great care is taken to have true surfaces at *x*. The follower

Mr. Robinson.

F is a thin steel plate, very slightly dished, so as to have a slight initial pressure upon the top of the rings. It is perforated, to let steam from A enter the space behind the spring, which it tends to push outward lightly during the up-stroke, comparatively strongly during the down-stroke. The rings are held against the lower flange of the piston body G, not only by the spring follower F, but by the steam pressure on the top of the rings; on the up-stroke, when there is less pressure above the rings, their friction against the cylinder is sufficient to keep them seated on the piston body. It will be noticed in this system, first, that the outward pressure on the rings *is relieved during the ineffective stroke*; and, secondly, that the principle of "constant "thrust" is applied to the rings as regards end-play. At all times, whether the piston moves one way or the other, and equally while it reverses its movement on the dead points, the piston rings are held closely down upon the piston flange, and there is none of that wear of the rings endways which tends to arise in all double-acting engines, owing to the friction of the rings causing them to lag behind, so that they are pushed one way by the piston body and the other by the junk ring or follower. Even if wear arose (under ordinary circumstances it cannot) the spring follower *would follow it up*: there would still be no hammering of the ring between the two sides of the groove. There is thus no increase of ring leakage; the longer the use the tighter the rings. The piston valves are on the same system as regards constant

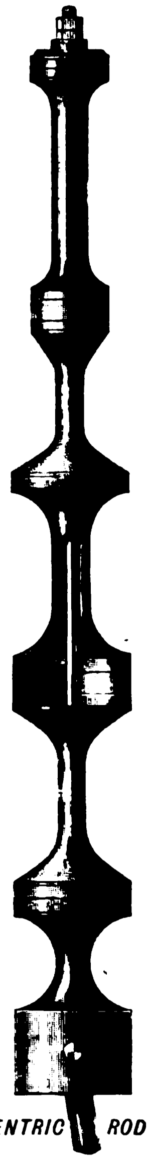


FIG. 3.—View, partly in elevation, partly in section, of a line of piston valves from a compound engine.

Mr. Robinson.

pressure of the rings and other parts against each other, but they are not steam-packed (see Fig. 3). They are separated by suitable cast distance pieces, with their ends faced to lie against the rings. All are strung together upon a steel tie-rod, with a kind of spring washer under the nut at the end. All parts are free to slide upon each other sideways; all are designed to be held together, without the possibility of hammering upon each other, by the pressure of the steam (in the steam-chest) upon the uppermost valve, aided by the spring washer referred to. The gland rings, used instead of stuffing boxes (see Fig. 4), are upon exactly the same system as

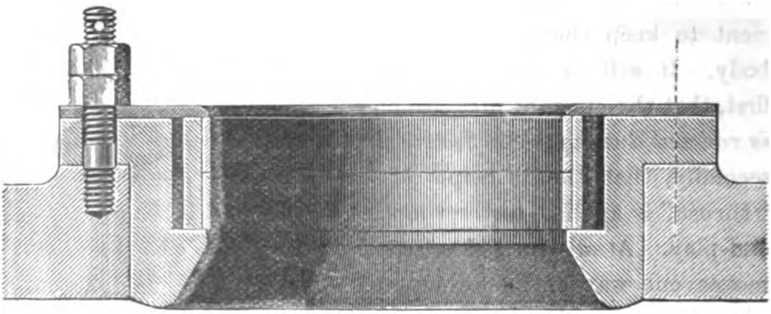


FIG. 4.—Sectional view of a Steam Gland Box.

the main piston rings; they are simply piston rings pressing inwards instead of outwards, and guarded against end-play in exactly the same manner. Steam is admitted behind them as in the main pistons.

13. This construction of the various piston and other rings is perhaps the most interesting application of the constant-thrust principle in the engine, but it has not been arrived at without long experiment and some changes. It was originally attempted to dispense with steam pressure behind the piston and gland rings—in fact, to exclude it; and many will remember the collapsing of the rings, especially when the steam was very wet, or when over-much oil was used. Under some circumstances a ring could always be set collapsing by increasing the oil supply: it is an old experience that water (or oil) will get behind a ring where steam alone will fail to pass. Once separated from

Mr. Robinson.

the cylinder wall, the ring was bound to spring inwards, until stopped by the butting of its ends: this butting and the subsequent spring outwards caused much noise and wear, and sometimes breakage of the ring, with, of course, waste of steam. Instead of a spring-plate follower, there was at first a fixed follower—which allowed end-play, and so wear, as in other engines—and afterwards a solid follower pressed down by spring washers, which also was not satisfactory. When "steam-packing" was first adopted, the holes in the follower were not made large enough to let the full pressure in behind the rings in the short time available, and so the system was imperfect. These defects have been corrected, and the system seems now to be not only perfectly successful, but in a sense beyond rivalry; for it would evidently be impossible to apply it in a double-acting engine—that is, in an engine where the preponderance of pressure changes from one side of the piston to the other—except by absolutely duplicating the packing arrangements of each piston; even then the conditions would not be quite the same. It may be added that very careful measurement, based upon some three years' experience, shows that the wear in the cylinders is less than with the earlier rings whose outward pressure was given entirely by springs. With rings which practically do not wear at all, and which are bound to keep tight whether they wear or not, it is not surprising that the economy of the engine increases with use.

14. **DRAINAGE** (*b*).—The best-drained double-acting engines are probably horizontal engines with separate Corliss or similar exhaust valves *beneath the cylinder*. The water lying on or draining down towards the cylinder bottom is pushed along by the piston until it reaches the exhaust port, but unfortunately only just before the moment of closing; however, it fills the port, and lies in it until the exhaust opens again—ready, it is to be feared, to rob the incoming steam of some of its heat during the next admission, and so to increase initial condensation. In vertical engines, at the bottom end of the cylinder the drainage may act continuously, so far as gravity can effect it, during the whole exhaust stroke, but in the upper end the water will rest on, and be carried up by, the piston until it rises above the port, when, as

Mr. Robinson. in the horizontal engine, only a very brief time is available for it to run away; much water probably lies permanently upon the piston, with injurious results from the point of view of initial condensation. If the exhaust port is also the admission port, the water cannot even lie there until the next exhaust; it is thrown back into the cylinder by the incoming steam. In none of these cases is the drainage satisfactory.

15. The drainage arrangements in the Willans central-valve engine offer a striking contrast to the foregoing (see Fig. 1). The piston is dished downwards in the centre,* not so much with the idea that the water lying on the piston will drain into the centre by itself, but to utilise the inertia of the water to drive it towards the centre during the rapid up-stroke of the piston. There is a ring of ports, HH, in the trunk or hollow piston-rod, flush with the surface of the piston, and of course moving with it, and *during the whole exhaust stroke* there is a rush of steam from the sides towards these ports in the centre, sweeping over the face of the piston and assisting the action last referred to. In fact, the exhaust port is *always* in the best possible place for drainage; it is as though the exhaust port in the horizontal Corliss engine moved along the cylinder bottom so as to be always just ahead of the piston. The water is out of the cylinder before the port closes, nor is there any clearance space in the port—that is, between the cylinder and the valve—where water could lie; there is practically no clearance. It is an ideal exhaust arrangement, and there is no apparent possibility of applying either it, or anything else as good, to a double-acting engine. It is probable that it involves a saving of about 15 per cent. in the steam consumption, in comparison with other vertical engines in which ordinary slide or piston valves are used, common to admission and exhaust. In Fig. 1 the central valve L is shown in its relatively highest position; the exhaust steam (and water) are passing away into the space B, as shown by the arrows, through the ports HH and KK. B is, of course, the receiver; it is in one sense the exhaust chamber of the working cylinder A, in another sense the steam-chest of the succeeding cylinder, if there be one.

* More particularly in the engines of recent design.

It will be understood that when the valve L is *below* the ports HH, steam passes into the cylinder from the upper part of the hollow piston-rod. Mr. Robinson.

16. **THE "CORNISH CYCLE."**—In the Cornish single-acting pumping engine the steam acts only on the top of the piston, the space below being at the time in communication with the condenser. To develop the power of the engine it would only be necessary to open communication during the up-stroke between the upper end of the cylinder and the condenser, and to maintain communication permanently between the lower end and the condenser: the piston would ascend *in equilibrio*, with vacuum on both sides of it. Viewing the upper end of the cylinder by itself, this is exactly what takes place in every ordinary double-acting engine, while the whole arrangement described, including permanent vacuum below the piston, is the most obvious and natural arrangement for a single-acting engine. In Watt's Cornish engine there is, however, another valve which cuts off the space below the piston from the condenser, at the moment when the steam above the piston commences to exhaust, and the exhaust takes place from above the piston into the space below it, and not direct to the condenser; the piston still ascends *in equilibrio*, but with higher pressure steam on each side of it. By this system Watt raised his pumping engine to a pitch of excellence never attained by his later double-acting engines; in fact, so economical (for a simple engine) is the old Cornish engine that in the controversies attending the introduction of the modern compound engine, the former was quoted to show that a simple engine could work as economically as a compound. The claim was excessive, except at very low pressures, but neither side realised that the Cornish engine is *not* a simple engine—it is half-way to a compound. Fig. 5 is a theoretical diagram from a simple engine in which the working cylinder opens alternately to the boiler and to the condenser. If it is also single-acting, and advantage is taken of that fact to introduce the

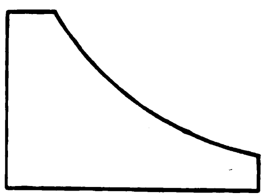


Fig. 5.

Mr Robinson.

Cornish cycle, the two diagrams given in Fig. 6 are obtained from the two ends of the cylinder, instead of the single diagram in Fig. 5. There is the same expansion in a single cylinder, but the range of temperature in it is only from admission temperature to release temperature, and not to condenser temperature. In other words, though the expansion is in one stage, the fall in temperature is divided over two: hence a great reduction in initial condensation, and marked economy.

17. It is obvious that with modern pressures it would be better to compound the engine—to give it one smaller cylinder and one larger, and add to the diagram the piece shown in dotted lines in Fig. 7, while making the division of temperatures more

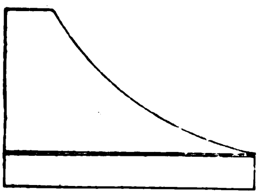


Fig. 6.

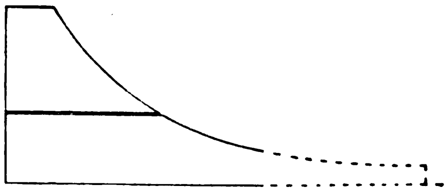


Fig. 7.

equal, as is also shown in Fig. 7. But whether the engine be simple, compound, or triple-expansion, if it is single-acting there will always be one cylinder, viz., the last of the series, to which the Cornish cycle may be applied. Mr. Willans applied it in his engine when circumstances were favourable, and he called it the "transfer plan." The steam, when it exhausts through the central valve from the upper to the lower part of the low-pressure cylinder, does not pass away at once to the condenser: it remains in the cylinder, and passes to the condenser *during the succeeding down-stroke.* It adds to the engine another stage of temperature-division, though not of large range, and so lessens the range in each of the others.

18. Simple as it sounds, the "transfer" adds several parts to the engine. The cylinders are lifted a good deal, as a "transfer "chamber" is required below the low-pressure cylinder; the trunks or hollow piston rods are lengthened; and an extra valve and an extra gland are required in each line, and of course involve extra friction. Thus it is only under particular conditions of pressure,

Mr. Robinson.

&c., that it is worth while to use the "transfer;" it is often better to add another complete stage of expansion rather than the transfer. It is, nevertheless, useful in cases where, for some extraneous reason, such as very great fluctuation of load, it is convenient to cut down the number of stages of expansion, as in an engine for electrical railway work. In such an engine the average load is usually but a small proportion of the extreme load, and the total cylinder volume should be so chosen as to give the best ratio of expansion, and therefore the most economical consumption, at the average load. It might then be impossible, without much complication, to obtain the extreme load at all, if the first cylinder were relatively so small as that of a triple-expansion engine must be, and if the cut-off in it, for normal load, were as late as in a triple-expansion engine it ought to be; it is better, therefore, to use a compound engine, gaining the needful expansion by cutting off early for average loads, and of course cutting off late (economically, too late) for extreme loads. Economy suffers from this, but it can be partly restored by adding another temperature-division stage in the shape of the "transfer." Where the steam-pressure exceeds 160 lbs. it is also worth while to add the "transfer" to a triple-expansion engine; it takes it half-way towards quadruple expansion, the latter not being, in a Willans engine, worth the complication it introduces, until the pressure is well over 200 lbs.

19. It will be noticed that the last statement is qualified by the words "in a Willans engine," and by this is meant that the Willans engine, even if the "transfer" is not used, has always one definite source of superiority over other engines working with the same number of expansion-stages (*i.e.*, compound or triple), namely, in respect of the smallness of its temperature-range in each cylinder. The explanation is that, in all the expansion-stages except the last one, *i.e.*, in the high-pressure and intermediate stages, the Willans engine works necessarily upon the Cornish cycle; the "transfer" is applied in those stages as a matter of course. In double-acting engines, the exhaust steam passes direct from one cylinder to the next, placing the two more or less in communication through the receiver, and therefore exposing the first of the two to the same temperature as the

Mr. Robinson. second; in the Willans engine, each cylinder exhausts successively in the Cornish manner, and the power developed in each is represented, as in the Cornish engine, by two diagrams—that from the cylinder proper, and that from the "receiver." The effect is that the temperatures of the high-pressure and intermediate cylinder, for instance, do not overlap, as in other engines they do, and the range in each is consequently smaller. It is perhaps the most distinctive feature in the Willans engine; it is highly advantageous to it; and it is of interest as a meeting-point between the very oldest and the very newest, and as bringing together in significant association the names of James Watt and of Peter William Willans—an association which to the author has always appeared natural, and which he believes will at no distant time be so recognised by all.

20. **BRAKE EFFICIENCY.**—Having fully dealt with the three special points of advantage peculiar to single-acting engines, enumerated in paragraph 11, it will be of interest to investigate the effect of the single-acting principle upon brake efficiency. Generally, a single-acting engine has higher brake efficiency, when fully loaded, than a double-acting engine. The perfect lubrication of all brasses and bearings in the splash chamber of a single-acting engine, naturally reduces that part of the friction to a very small figure, and the total absence of back-lash, or "lost motion," is believed to tell as strongly in the same direction. On the other hand, single-acting engines, or at least the Willans engines, though they have no stuffing boxes, have unusually numerous piston and other rings, mainly owing to the practice of putting a complete tandem engine over each crank—or three cylinders over each crank, if triple expansion. Nine complete cylinders,* as fitted in three-crank triple-expansion engines, with their valves and glands, imply a large number of rings, and even in a two-crank compound engine there are a good many. The special design of the rings, which

* *Twelve* complete cylinders, has been unkindly said, reckoning the guide or cushion cylinders. But the reproach is not quite just, since the guide cylinders, with their ring-less pistons, merely represent the guides and cross-heads of other engines, and are quite as simple.

has been fully described in paragraph 12, coupled with accuracy in manufacture, bring the total friction, however, to a reasonably small amount. After some months' running, it is not uncommon to find the brake efficiency of compound engines as high as 92 per cent., but on trial at the works, when the rings are new and not yet brought up to a good face, it is rare to get more than 90 per cent., and 89 per cent. would not be thought unsatisfactory. In a triple-expansion engine 87 or 88 per cent. is a good result when new. Though experiments are sadly wanting, it is believed that these figures are much about the same as those for good double-acting engines, in which there is less ring friction but more bearing friction. Mr. Robinson.

21. Mr. Willans's diagram of total water consumption, shown in the discussion upon Mr. Crompton's paper upon the Cost of Electrical Energy,* has familiarised every one with the rather puzzling fact, that in almost all trials at Thames Ditton, with good dynamos, the lines representing total water per electrical H.P. and per indicated H.P. remain nearly parallel for all loads; in other words, the combined engine and dynamo losses, and *a fortiori* the engine losses alone, are as great at light as at heavy loads. Makers of some double-acting engines are believed to have obtained results differing from this, and showing a considerably reduced engine friction at light loads. Probably both are right. In both types the ring friction is constant. In the single-acting engine the bearing friction is also practically constant, because its maximum is so small that variation in it is unimportant. In the double-acting engine the bearing friction† at full load is relatively great, and as it falls off at light load, it considerably reduces the total friction. If the single-acting engine beats the other handsomely at full load (as it often does), the effect may

* *Proceedings of the Institution of Civil Engineers*, vol cvi., 1890–1.

† In "bearing friction" is here included that which is assumed to be due to back-lash, *i.e.*, to the incipient "knock" necessarily present where there is alternation of stress. It is to be remarked that the heat generated by knock upon brasses is wholly lost; but heat generated by piston-ring friction is partly utilised in heating the cylinder walls and pistons, and so reducing condensation. In the ingle-acting engine, therefore, a greater proportion of the friction loss is returned in reduced consumption of steam, than in a double-acting engine having the same total friction.

Mr. Robinson.

only be to bring them together at light load; but where the double-acting engine is exceptionally free from friction, and makes a more even fight with the single-acting engine at full load, it may have a distinctly higher efficiency at light load.

22. The value of improved efficiency at light loads is not arge, for the unwisdom of running an engine at light load, for any permanent use, is too great to be redeemed by any probable saving in engine friction. If light loads have to be faced, there should be a smaller engine to undertake them, small enough to make a full load of them, or nearly so.

23. The possible higher efficiency of double-acting engines at light loads is well illustrated in a paper read by Mr. Alexander Siemens before the North of England Institute of Mining and Mechanical Engineers, in December last. It describes fully a series of trials in the central power-station belonging to his works at Woolwich, between a Willans engine (3-crank) of 300 I.H.P. and a Belliss double-acting engine (2-crank) of the same power, and running at the same speed—350 revolutions per minute. The dynamos were apparently identical, and all the conditions appear to have been as favourable to a fair test as they can ever bein a commercial engine-room. There are anomalies in the very interesting figures given, but there is no reason to mistrust their general tendency. At 270 I.H.P., the highest power used, the combined efficiency of the Siemens-Willans plant was 86 per cent., against 85·1 per cent. in the Siemens-Belliss; but at light loads the advantage of the latter was considerable, as appears below:—

——	Electrical H.P.	Indicated H.P.	H.P. lost in Engine and Dynamo combined.
Willans Set ..	231·2	268·9	37·7
Belliss ,, ..	230·4	271·6	41·2
Willans Set ..	114·6	149·2	34·6
Belliss ,, ..	120·3	144·7	24·4
Willans Set ..	60·9	95·7	34·8
Belliss ,, ..	60·5	79·4	18·9

Mr. Robinson.

There is much in these figures which is with difficulty reconciled with any probable assumption as to the respective engine and dynamo losses; but there is no doubt that in the double-acting (Belliss) engine the friction became very small at light loads. As it had but two cylinders and ran at high speed, there could be but little ring friction at any load, and it looks as though, *at light loads*, the bearing friction almost disappeared; probably the crank-pins and shaft "floated" in their brasses, permanently separated from them by the oil which was forced in under pressure. It is, however, instructive to notice that with increasing load, and notwithstanding its great advantage in regard to ring friction (it probably had little more than one-third as much ring friction as the other engine), the total friction of the double-acting engine rapidly overtook that of its rival, and it is evident that had trial been made at the much higher mean pressures frequently used in non-condensing stations, or even at the intended normal full load of the engines actually tried, the advantage of the single-acting engine would have been very marked indeed. It seems possible that, although the exceedingly elegant system of lubrication adopted in this double-acting engine is effective when the pressure per square inch on the brasses is light, yet the alternating pressures of the parts, and the knock to which those alternations give rise, whether it be great or small, begin to produce their natural effect at heavier loads. In the single-acting engine security from wear and knock is obtained by ensuring that the revolving journal shall remain always in steady contact with *the same* brass: in the remarkable double-acting engine described, an even more perfect result is perhaps obtained *at light loads* (which, however, are not of the highest importance) by providing that the revolving journal shall touch *neither* brass; but it remains to be seen whether in continuous running at heavy loads, the brasses will not relapse more or less into the condition of those of an ordinary double-acting engine. It is at least plain from the Woolwich figures that even at 270 I.H.P. the bearing friction (as distinguished from ring friction) of the double-acting engine is enormously greater than that of the

Mr. Robinson. single-acting engine, and the law of its increase might well repay investigation. Provisionally, it is difficult to resist the conclusion that bearing *friction—i.e.*, the actual rubbing of the journal upon the brass—is of small importance in engines in which the lubrication is very perfect, as in the Willans engine through its oil bath, or in the Belliss engine through the forcing-in of oil under pressure;* and that, in double-acting engines, at heavy loads, the most important element of friction-loss—or, it may be safer to say, of power-loss—is to be traced to the knock of the journal as it changes over from one brass to the other—a source of loss which the single-acting (constant-thrust) engine avoids altogether. Whatever be the ultimate verdict, no one can withhold admiration from the ingenuity, and indeed beauty, of the device by which it is sought to overcome so great a difficulty; and it is most satisfactory that in such an interesting and important trial, the double-acting principle was represented by so perfect an example.

---

24. Passing to the results of experience with single-acting engines in the last few years, it may be well to deal separately with—

(*a*) Steam Consumption.
(*b*) Durability.
(*c*) Lubrication.
(*d*) Vibration.

25. **STEAM CONSUMPTION** (*a*).—The consumption of the best-known type of single-acting engine, under all loads, is known not only from the almost historic series of tests conducted by Mr. Willans, but from numerous other trials carried out at Thames Ditton. It is, however, a frequent objection that the small consumption of steam per kilowatt upon test, bears little rela-

* If the mere rubbing of the journal and brass together, at heavy loads, were seriously important in a thoroughly lubricated engine, the Willans engine would show increasing friction at heavy loads. It does not do so, and even the increase (in engine and dynamo together) shown in the figures extracted from Mr. Siemens's paper is exceptional. The increase in the Belliss engine, therefore, can hardly be due to this cause, and other reasons must be found for the increase which undoubtedly occurred.

tion to the actual consumption of coal per kilowatt sold; and doubt is often expressed whether the engine consumption does not increase with use and wear, or whether test figures are not vitiated later by variation in load. It is satisfactory to be able to answer this question. At the stations of the Westminster Company, Dr. Kennedy, F.R.S., has for a long time had accurate records kept of the quantity of water supplied (by meter), as well as of the load upon each engine separately. The "Thames Ditton" consumption for every engine at all loads is, of course, known, and has been compared with the actual consumption, and after allowance for steam known to be condensed in pipes and used in pumps, &c. (mostly ascertained by experiment), the result is *an almost exact balance.* Probably there really are unrecorded losses for which a margin would have to be provided, if it were not that the engines work at a little less than the calculated consumption, since the latter is based upon the trials of the same engines at the makers' works, when new and stiff. Some of the engines had been at work for over five years, and few for less than two years, so all had had time to bring the rings and other wearing surfaces into the best condition for economy, with the almost certain result of improving upon the Thames Ditton trial figures. Mr. Robinson.

26. At the meeting of the Institution of Mechanical Engineers on the 24th April, 1895, the above facts were well shown in a diagram exhibited by Mr. G. M. Clark. Two cases were taken, from the same station: one for a heavy winter load, covering 24 hours; the other for a week's load in summer. The leading particulars were:—

Mr. Robinson.

	Winter Load (24 hours).		Summer Load (one week).	
Output in kilowatt-hours	3,350		9,000	
Measured total water-consumption, per kilowatt-hour, including all station losses ... ... lbs.	...	**46**	...	**48**
Measured steam-consumption of engines at Ditton trials (full load, non-condensing), per kilowatt-hour ... . lbs.	32·5		32.5	
Calculated addition to steam-consumption (non-condensing) on account of low load-factor, *based on actual tests at Ditton**... lbs.	8·0		9·5	
Total calculated engine consumption, per kilowatt-hour ... lbs.	40·5		42·0	
Measured consumption of steam in pumps, per kilowatt-hour... lbs.	1·5		1.5	
	42·0		43·5	
Balance, attributable to condensation in pipes, leaks, &c., ascertained by previous trials to vary from 7 % to 11 % of total consumption ... ... lbs	4·0		4·5	
		46		**48**

These figures may be commended to those gentlemen of the technical Press who tell us that theory never helps anyone; that no engine-builder knows within pounds what weight of steam his engine is likely to use in practice; and who apparently reckon no man practical if he knows the reason for what he does or is able to foresee the result.

27. It is important to inquire whether the economy of the single-acting engine may be expected to make any notable advance beyond the results hitherto obtained. The answer is,

* The words in italics were added in accordance with the suggestion contained in Dr. Kennedy's letter (see page 537).

Yes; but here (speaking of the Willans engine only) it is necessary to distinguish between actual engines as fitted in the majority of stations, and the new type only recently made. Owing to various circumstances, the demand for the central-valve engine came at first chiefly from users who desired to work without condensation, and its fame was first established by Mr. Willans's paper upon Non-condensing Trials, in which he showed figures not before supposed to be possible for non-condensing engines (such as a consumption of 18·45 lbs. of steam per I.H.P. with 170 lbs. pressure, triple-expansion; and 19·45 lbs. with 150 lbs., compound). But Mr. Willans's paper, and his practice at that time, did more than this—they pricked the bubble of over-expansion. Here was a little non-condensing engine which, with five or six expansions, used scarcely more steam per I.H.P., and obviously less per brake H.P., than many large condensing engines which expanded their steam twenty-fold or even thirty-fold. But the ease of the triumph tended to mislead, and the same ratio of expansion, without the moderate increase which was really desirable, was provisionally attempted in condensing work. A few months before his lamented death, Mr. Willans carried out a complete series of condensing trials, which proved that, while nothing could justify the excessive expansion formerly allowed by some makers, the Willans engine, as then made, erred, though far less seriously, in the other direction. A new series of engines has since been designed in which the necessary greater expansion is provided for, the ratio of the cylinders being altered at the same time; and in which a number of minor improvements, tending to the suppression of petty losses, considerable in the aggregate (but much less important in a non-condensing than in a condensing engine), have been introduced. From these engines, which have but just commenced to find their way into station work, great economy may be expected. The earliest was a triple-expansion mill engine indicating up to about 500 H.P., driving the flax mill of Messrs. Gunning & Campbell, Limited, at Belfast, at 300 revolutions per minute; and in some very complete trials carried out last year by their consulting engineer, Mr. A. Basil Wilson, M. Inst. Mech. E., after the engine had been at work for Mr. Robinson.

Mr. Robinson.

about six months, a consumption of 12·48 lbs. of steam per I.H.P. was recorded. This places the single-acting engine, to say the least, in the very foremost rank of economical motors. Evidently electrical engineers need not fear that they must abandon economy if they do not "stick to the builders of mill engines and marine "engines." In fact, it seems not impossible that, while advocates of the "mill engine" are laying claim to central-station work, they may find themselves outflanked, and may learn too late that mill engineers and owners have become as open-minded as electricians, even to using "electric light engines" for driving their mills.

28. It may be remembered that in Mr. Willans's paper on Non-condensing Trials, figures were given for one condensing tria at 170 lbs. pressure, carried out with the same triple-expansion engine as had been used for the non-condensing trials: the consumption was 15·1 lbs. per I.H.P. per hour. With the old engine that result has not been much improved upon, so it may be reckoned that the new series are about 15 per cent. better in steam economy than the old, *when condensing.* For non-condensing work, and for condensing work in special cases, the old-series engines are still the most suitable, while from the smaller cylinder capacity they are less costly to make. The engine at Messrs. Siemens's works at Woolwich, the trial of which against a Belliss engine has been before referred to, was of the old pattern, compound, but fitted with the "transfer" arrangement: in other respects it was of the proportions now recognised as suitable for non-condensing only. The steam consumption at full load is given by Mr. Siemens, in his paper before quoted, as 19·5 lbs. per I.H.P., against 22·4 lbs. in the Belliss engine. It is understood that these figures of steam consumption are based upon the total water actually supplied to the boilers, with a deduction only for steam used in pumps; they presumably include pipe condensation and leakages of all kinds, and it is probably from some such causes, rarely absent from a commercial engine-room, that the water used comes out some 2 lbs. per I.H.P. per hour above that due to the proper steam consumption of the old type Willans engine at the load and

pressure stated. (The vacuum is not given.) Of course the Belliss engine was similarly affected. Mr. Robinson.

29. Reference has been made, in paragraph 20, to the numerous cylinders of the Willans engine, due to the practice of erecting a complete engine over each crank. By adding to the number of pistons and valves the ring friction is, of course, increased, and it is equally certain that condensation is increased, and economy affected unfavourably, owing to the larger aggregate surfaces of the two or three small cylinders which take the place of what might otherwise be a single large one. On the whole, however, the advantages appear to outweigh the drawbacks. The first and most obvious is even turning moment. Whatever the change in load, in pressure, or in cut-off, it is at least certain that the cranks will be driven equally, and this is an advantage not to be lightly surrendered for only a small gain in economy. But the same tandem arrangement of cylinders is also the condition of the complete application of the Cornish cycle to all the cylinders except the low-pressure one: the perfect separation between the temperature ranges of the successive cylinders is impaired if their strokes do not succeed each other at intervals of exactly a revolution—as they now do in the tandem pattern; even economically, therefore, there is something to be said in favour of the many small cylinders. Turning to other considerations, it may be thought at first that the large number of small cylinders and multiplication of parts, must lead to increased weight and cost, as well as to difficulty in dismantling and overhauling. As to weight, it is almost certain that it is not increased. Cylinders weigh roughly in proportion to volume, but the weight of a piston tends to increase faster than the area; three pistons, each of one square foot area, will weigh less than one of three square feet area. To cylinder covers the same consideration applies. Piston-rods, connecting-rods, and crank-shaft should be unaltered. There may be some saving in machine work, but small pieces are after all easier to machine than large ones, and pieces which are made in batches of, say, 36, are more economically manufactured than larger pieces made in batches of 12 only. Then the identity of the several lines of

Mr. Robinson.

parts, and their interchangeableness, are often helps to the user (especially in case of repairs), while the lightness of the individual pieces and the regular and simple routine in removing them make dismantling easy. A Willans cotton mill engine of 650 I.H.P. pulled up one day after the dinner hour, with brasses cut to pieces and crank-shaft badly scored. (Of the cause, it need only be said that emery powder was found in the bearings and in the crank-chamber.) *Next morning*, after breakfast, it started again with new brasses, and crank-shaft in good order. By good luck another engine of the same size was being erected in the same town, and its brasses were at once appropriated for the disabled engine, and every necessary repair thoroughly carried out during the night. It is true that in this case the cylinders did not require to be lifted, but there have been as rapid repairs to cylinders, pistons, &c., where internal damage has been done by water. Some years ago, three 3-crank triple-expansion engines of 300 I.H.P., to save the reproach that they had nine cylinders each, were designed with only six cylinders, viz., one high-pressure, two intermediate, and three low-pressure. It is remarkable that not only have all later engines of the same type been fitted with the orthodox nine cylinders, but the original engines in question are now being altered to the standard pattern. With larger sizes may, possibly, come different views, but as regards the present standard patterns for 700 or 800 I.H.P., experience gives little support to objections against the number of cylinders.

30. A discussion upon the economy of the single-acting engine should include running expenses other than steam, but this would greatly lengthen the paper. Moreover, it is only users who can pronounce upon this question finally and with authority. That single-acting engines of the closed type are very economical in oil is generally allowed, and economy in attendance is also conceded. An engine which oils itself cannot want much looking after. Repairs are the only point which really admits of dispute, and these are dealt with under "durability" in the succeeding paragraphs.

31. **DURABILITY.**—The idea that because a machine makes

many revolutions a minute it must wear out more rapidly than a slow-running machine, seems to have the force of an axiom for some minds. Yet there is nothing axiomatic, nor even, to the instructed, probable about it. In the first place, high speed is a very winnowing fan, in the matter of workmanship. It is surprising what a slow-speed engine can endure, for a season, in the way of bad workmanship; but not so the high-speed engine. In the next place, the piston speed is usually much lower than in slow-running engines, owing to the short stroke:—about 525 feet a minute is the highest piston speed in any single-acting electric light engine. The wear of piston rings and cylinders is bound, other things being equal, to be proportional to the piston speed, and this applies also to valves and glands. In one important point, however, other things are not equal, but are largely in favour of the single-acting engine. The system of packing rings described in paragraph 12, which cannot be used in double-acting engines without much complication, has decisive effect upon cylinder wear, and even if one went back to the records of the old vertical beam engines, which used steam at 30 lbs. per square inch, or less, and have been described as "revolving "occasionally," it may be doubted whether such evidence of good wear could be found as is now to be quoted in favour of the fastest running engines. Mr. Robinson.

32. The makers of the Willans engines have for some years past lost no opportunity of obtaining and recording exact measurements of wear, where they have been called in to carry out overhauls or examinations. The results are systematically recorded, and they show, speaking generally, an absence of wear beyond all claims made or expectations formed by those interested in the engine. On March 9th, 1895, a G.G., or 80 I.H.P., compound non-condensing engine, No. 906, used for lighting one of the largest hotels in London, and stated to have run (at about 400 revolutions per minute) for 14 hours per day, including Sundays, was taken down for overhaul—never having been so much as taken down for examination since its erection in April, 1890, that is, *five years* previously. The report states that "all the parts were carefully measured, and the H.P. and

Mr. Robinson.

"L.P. trunks showed no perceptible wear, and were replaced the "same as they were taken out. On measuring the H.P. cylinders, "we found they were 2–1,000ths of an inch larger than our "standard size, and the L.P. cylinders were the same; still they "were quite round. All piston rings and gland rings were replaced "without any repairs whatever. The whole of the repairs to this "engine, including taking down and re-erecting, were executed in "24 hours."

33. It will be justly said that if such a report reflects great credit upon the engine, it reflects no less credit upon the engineer who has charge of it. The constant and heavy work in this case was no real disadvantage: the chances of corrosion during stops of a few days are much more injurious. In a lighting station where work is more intermittent, and where during the summer engines are liable to be laid off for comparatively long periods, the wear (or corrosion) is greater, and the following is an instance of what is looked upon as heavy wear under such circumstances. The engine is of H.H.-12 size, No. 999, being of an exceptional type—that is, having 12-inch stroke instead of 8-inch (as usual in H size), and running under 200 revolutions instead of at about 380; it was one of a pair ordered to a special specification, and from the point of view of objectors to high speed it should give good results. It was erected in March, 1891, at one of the London electric lighting stations, and in February last it was taken down for an ordinary overhaul, and was re-erected. As the condition of the engine was manifestly good, and as it was non-standard, so that information about it had not the usual value, it was not measured; but, hearing that a visitor to the station had measured the H.P. cylinders while lying about, and had stated them to be much worn and considerably oval, the makers, though certain that the statement was a mistake, asked permission to take the engine down again, and made unusually full measurements, checked by more than one observer, in the presence of the chief engineer of the station. It appeared that in one cylinder, though the ovalling ranged only from 2 to 7-1000ths, there was in one part wear (in four years) of 28-1000ths, or nearly 1-35th part of

an inch; the other cylinder showed about half this wear, and no ovalling.* Mr. Robinson.

34. The third instance is of an engine working under really good conditions—that is, driving a cotton mill, and working therefore for long hours, and with scarcely any stops exceeding two days. This engine, No. 1,253, of 650 I.H.P., commenced work in April, 1892, and though the cylinders had been taken down before, they had not been closely examined or measured. At Easter, 1895, it was thought desirable to have the engine closely examined, and between the Thursday evening and the following Sunday afternoon it was completely dismantled, thoroughly examined and measured over, cleaned, re-erected, and started under steam. There was no repair of any kind to be done, and but for the accidental breakage of some studs during re-erection it would have been running a few hours earlier. The report upon wear was most satisfactory, but the facts are best given in the words of the inspector sent by Mr. Michael Longridge, who supervised the examination on the part of the owners of the mill. This gentlemen wrote:—

"The cylinders were taken down and pistons and valves out; " the cylinders are 17 in., 28 in., and 40 in., with 1 ft. 6 in. stroke. " Revolutions, 206; boiler pressure, 160 lbs. I examined and " gauged all the six cylinders with a very fine gauge, one that can " be set to 1-10,000th part of an inch, and found H.P. cylinder " about 1-1,000th part of an inch worn; I.P. cylinder 1-3,500th " part, and L.P. 1-7,000th part of an inch worn; indeed, the tool " marks had not properly worn out. The surfaces as smooth as " glass. The pistons and rings seem to be very good, and the " rings of the piston valves also appear to be very good, all

---

* The statement which gave rise to the re-measurement was that one cylinder was fully an eighth of an inch oval, and that there was much tapering and wear generally. It was made beyond question in absolute good faith, probably from using inadequate or extemporised measuring appliances. The remarkable point was that the visitor was an engineer, and stated that he had been provoked to measure the cylinders by being told that no wear would be found in them, but that, "as an "engineer himself, and accustomed to measure cylinders," he did not consider the wear, *as he supposed it to exist*, was excessive for the time the engine had been running.

Mr. Robinson.

"bearing well round; the piston-rods or trunks also appear "to have good surfaces; also internal surfaces, where the valves "work, seem good, and the rings of the piston valves also appear "very good, so that they scarcely appear to have been at any "work. I must say that I never came across anything so "accurately fitted together; even gauging the cylinders with "the above-named fine gauge, I cannot say that I even found "a slack place one way from another. Whether the engines "are of good principle or not, the workmanship is good, which "contributes to small wear and little friction Crank-shaft necks "and crank-pins seem very good, being very smooth." "Whether "the engines are of good principle or not," is valuable; clearly the giver of this emphatic testimony is no devotee of the single-acting engine. All brasses and journals, eccentrics, &c., were stated by the overhauling staff to be in perfect condition.

35. When the single-acting engine has run n years, it is always open to objectors to say (as they do) that no doubt it is good for n years, yet $n + 1$ years will finish it. The argument admits of no refutation; that is, the present objectors may be right—for a year, though those of last year are refuted already. But reasonable men with such evidence as the foregoing before them will pay more heed to the counter-claim, which is, that the durability of the high-speed single-acting engine is not inferior to, but enormously exceeds, that of the best double-acting engines, unless the latter revert to an archaic slowness of running, when their enormous size and cost will exclude them from practical consideration.

36. With many hundreds of single-acting engines at work, there are, of course, some cases of trouble. There have been broken crank-shafts, though not in an unusually large proportion, if we exclude a succession of breakages in a single important station which attracted much attention. There were special reasons in that case which contributed to the result, but the plain and honest reason why the cranks broke (as with all other cranks which have broken since cranks began) is that they were simply not strong enough. The engines (200 I.H.P.) which formerly had a 4-inch shaft have for some years past had 5-inch

shafts. The 4-inch shafts have broken, and the 5-inch have not, and are not likely to—apart from the effect of those occasional flaws against which no steel forging can be absolutely guaranteed. Mr. Robinson.

37. Other cases of excessive wear, or of damage, when not referable to defective lubrication, may usually be traced to underloading, *i.e.*, to running with so light a load that the pressure in the steam-chest becomes insufficient to hold down the lines of valves in constant thrust against the eccentrics. The single-acting engine has, of course, the defects of its qualities. Constant thrust is perfect so long as it lasts, but without it, the single-acting engine has no claim to admiration. For the pistons, and main moving parts generally, the question was settled once for all, so far as regards the Willans engine, by the well-known "air-buffers," which provide completely for cushioning, irrespective of change either of load or of back-pressure, the distribution of the steam by the valves being absolutely untrammelled by any consideration of cushioning. Unfortunately, the air-buffers do not cushion the valves, and it is as important that the latter should be held closely down upon the eccentrics as that the connecting-rods should not leave the crank-pins. The line of valves is fairly heavy, and owing to the eccentric being seated on the crank-pin, though the stroke of the valves relatively to the ports is moderate, yet the total stroke is nearly as long as that of the pistons. "Holding-"down power" is intended to be given by the pressure of the steam in the steam-chest upon the topmost valve, but if the pressure is too much throttled down, the "constant-thrust" element is wanting, and the parts have to be checked upon the up-stroke, more or less, by the underside of the eccentric strap bearing against the sheave—a state of things which, at least in theory, is inadmissible in a constant-thrust engine. In practice, however, if there is good surface on the eccentric sheave, and a well-hardened pin at the small end of the eccentric rod, no serious results follow, provided the defect of pressure in the steam-chest is only occasional, and is not too considerable. Under such circumstances, however, it is necessary to look now and then to the eccentric straps, and take up the

Mr. Robinson.

wear if necessary, and to be warned in time if the engine begins to run noisily. The lesson of the wastefulness of running an engine at light load, if it can possibly be avoided, has now been so well learned, that in lighting stations at least (except, perhaps, in alternating stations where parallel running is not practised), there is little danger of the load, and consequently the steam-chest pressure, being allowed to fall low enough to jeopardise the "constant-thrust" of the eccentric straps; but where such a probability exists it adds a reason for using variable expansion gear, which of course secures the maintenance of high pressure in the steam-chest. In a throttling engine it is also easy to secure the required holding-down pressure on the valves by forming the little steam-chest dome which tops each line of parts, as a cylinder, in which the uppermost of the line of valve-pistons works. The top of each little cylinder is in communication with the steam pipe outside the throttle valve, and so gives a constant down-pressure on the valves, irrespective of throttling. Of course these fittings cost something, and they need special lubrication, so that it is only in very exceptional cases that it is worth while to fit them, and in such cases there are probably other sufficient reasons for using variable expansion gear instead. It is well to bear in mind, however, that in special cases, the durability of the valve-motion may require to be secured by special precautions; it is not absolutely assured under all circumstances like that of the main working parts.

38. **LUBRICATION** (*c*).—Where the parts of an engine lie permanently in the lubricant, both when running and when standing, the quality of the oil is of more importance than where it is used in the old-fashioned way. Free acid in oil, which is more common than is usually supposed, leads to corrosion, and in some few cases has caused serious trouble; the quest for a perfect crank-chamber lubricant, which shall always be the same, is not quite ended yet. Cylinder lubrication has long ceased to trouble (if the user does not sacrifice everything to cheapness), but not so the other. Sometimes it would appear that acid is brought over in priming water from the boiler: in some cases

it has been traced, with tolerable certainty, to special boiler compositions containing acids, which may find their way with priming water past the valves into the crank-chamber. The tests for acid, however, are comparatively easy, and its presence is soon shown by the rusty red colour which the lubricant begins to assume, and, in worse cases, by the condition of the parts. Prevention also is usually easy, as by the use of small quantities of soda, or of soft soap, added daily to the crank-chamber in very minute quantities: if there is too much of it, it "lathers" so successfully that it drives everything else out of the crank-chamber. The "personal equation" enters much into this, as into other questions of engine management. There are many hundreds of closed engines running where trouble with lubrication has never been heard of: yet in a few, working apparently under identical conditions, everything is different. Where oil is of doubtful quality, temperature plays, or may play, an important part, and the advantage of keeping the crank-chamber cool has for some years been recognised. In the Willans engine, cooling pipes are usually fitted in the crank-chamber, through which, if the engine works non-condensing, it is well to run a small stream of water: about one-fourth of the quantity which is supplied to the engine as steam, per hour, is generally enough, but it is sometimes a convenient plan to run all the feed water through. In a non-condensing engine the crank-chamber is hotter than in a condensing engine, because the main source of heating is conduction, by the walls of the crank-chamber, from the "exhaust-chamber" above. When the exhaust temperature is about 215°, as in a non-condensing engine, the crank-chamber, of course, grows hotter than where the exhaust is at condenser temperature. 170° Fahr. is a perfectly safe temperature for the crank-chamber, but the lower it is, the more one has, so to say, in hand, and it is such an advantage to have complete control over the temperature, that it is generally worth while to adopt the cooling pipes, even in condensing engines. The large ones are made, as shown in the section in the Appendix, with a kind of water jacket to the crank-chamber base. It is evident that the engines might be constructed with a more complete separation between the hot cylinders and

Mr. Robinson.

Mr. Robinson.

exhaust chamber, and the cool crank-chamber: there might, for instance, be an air-space and columns between those parts. But, as is true of many other possible minor improvements, the gain from this arrangement might be bought too dearly: it is cheaper to take easy precautions against small and infrequent troubles, than to make the engine larger and more costly, even if theoretically more perfect.

39. **VIBRATION** (*d*).—The increase of town lighting stations gives great importance to this subject, but as it has recently been somewhat fully dealt with, especially as regards marine engines, in a paper contributed by the author, jointly with Captain Sankey, R.E., to the Institution of Naval Architects, it is not proposed to treat it at length on the present occasion; it will suffice now to say only enough to show in what direction improvement is most probable.

40. In a recent well-known case, several single-acting engines, supposed to be remarkably free from vibration, have been removed from a London station, and replaced by steam turbines, as the only certain means of meeting the objections of persons living close by. What was the cause of the vibration, and how can it be avoided, without abandoning altogether the accepted reciprocating form of steam-engine?

41. The station contained ten engines of 200 I.H.P. each, and it is no exaggeration to say that a person standing on the great concrete block which formed the foundation for all the engines, could scarcely tell whether those near him were running or standing. Yet complaints from houses at some little distance (rather than from closer neighbours) were evidently well founded. The engines had two cranks each, 180° apart, as shown in Fig. 8, where there is an obvious couple, AB, which tends to set the engine rocking endways, and to give rise to vibration. But it was incredible that the huge block of concrete, measuring 88 feet by 24 feet by 7 feet, could be tilted crossways by the couple between sets of pistons moving in lines only 2 ft. 3 in. apart.

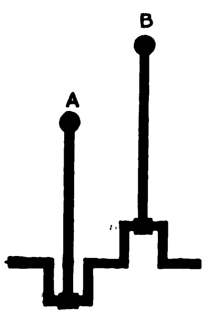


Fig. 8.

Mr. Robinson.

Besides, the effects produced, and the known nature of the subsoil, led to the belief that the block was not so much rocking as lifting up and down bodily, and acting like a pump upon the water-laden soil below (the complaints were most serious after periods of heavy rain). It would have been easy to balance the "couple" if that alone were the cause, by dividing one of the lines of pistons into two, each of half the weight, and using cranks as in Fig. 9, where B and B[1] are both at 180° from A, and the

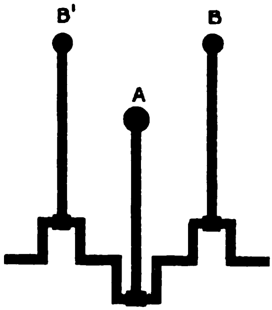


Fig. 9.

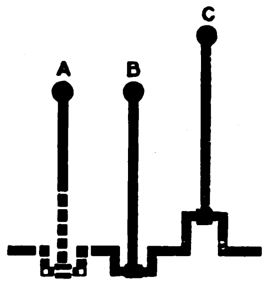


Fig. 10.

parts connected with them are collectively of the same weight as the one line connected with A. This plan is described in Lowrie's Patent No. 3,040 of 1885, and it has also attracted attention recently. The engine shown in Fig. 9 would have no tendency to rock endways, but it was soon seen that it would effect no improvement in the station referred to, for on closely examining the matter mathematically (in February, 1893), it was found by Captain Sankey that in the engines in question, and broadly speaking in all vertical engines, the tendency to rock endways, by reason of an unbalanced couple, was much less important than the tendency of the engine, as a whole, to move vertically up and down, through a cause depending upon the obliquity of the connecting-rod. Previous calculations had usually been addressed to balancing the couple, as a practical solution of the whole vibration question, and though it was known that the obliquity of the connecting-rod had an effect on the result, it was believed to be disposed of by the statement that "the slight and immaterial errors due to the

Mr. Robinson.

"finite length of the connecting and eccentric rods need not be "considered."*

42. The engines were central-valve engines of I.I. size, having lines of parts each weighing 420 lbs. They ran at about 350 revolutions per minute, and had a stroke of 9 inches; the ratio of connecting-rod to crank was 4·77 to 1. The two points of maximum piston-velocity are of course above half-stroke, hence the changes of piston-velocity, and therefore the inertia forces, are greater in the upper half than in the lower half of the revolution. Without going into details, it appeared that when each line of parts was near the top-centre, it exerted an upward pressure upon the engine of 3·54 tons, but when near the bottom centre, it pressed downwards with only 2·31 tons. AB, in Fig. 8, therefore represents not only a couple but a force, for A and B alternately exceed each other by about 1¼ ton, twice per revolution. Twice per revolution there is a force of 1¼ ton trying to lift the engine; and twice per revolution this changes into a nearly equal force acting downwards. (The inertia curves establishing these results are given with the paper before mentioned.) A number of engines developing these unbalanced forces, and synchronising every few seconds, were probably well calculated to produce the results complained of. Needless to say, the engine shown in Fig 9, if comparable with that in Fig. 8, as regards revolutions, stroke, and total weight of moving parts, would give exactly similar results in respect of the unbalanced vertical inertia forces acting upon the engine as a whole.

43. At the same time the inertia forces in a three-crank (I.I.I.) engine, having the same revolutions, stroke, and weights of parts, but with three cranks at 120° instead of two at 180°, were investigated, and it was found that the unbalanced vertical forces entirely disappeared, or, rather, so nearly disappeared that they amounted to about a pound only, as against 1¼ ton either way, or 2½ tons in all. It was concluded by the makers that if three-crank engines were substituted for the two-crank engines, the vibration difficulties would be ended; but the engineer of the lighting company naturally hesitated to adopt a remedy based chiefly upon calculation, for, although such three-crank engines were

* Quoted from a patent specification.

running elsewhere, and with remarkable absence of vibration, there was nothing to show with absolute certainty that the mathematical reason was the right one. He therefore shifted the engines to another station, and put down steam turbines instead, feeling sure that they would at least secure the object of getting rid of vibration. The result of this investigation was naturally to give an impulse to the use of three-crank instead of two-crank engines for lighting stations. Mr. Robinson.

44. In 1894, the proprietors of the Heilmann system of electric traction were in search of vibrationless engines to mount upon two very powerful electrical locomotives they intended to build, and they applied to the author's company. The engines were to be of 1,400 horse-power each, and as they were to stand fore-and-aft on the engine framing, and as no weight could be allowed in the nature of "foundation," it was desired to eliminate the "couple" as well as the vertical inertia force. This, it is hoped, will be effected by the design shown in Figs. 11 and 12,

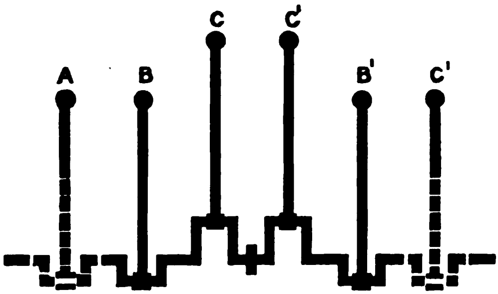


Fig. 11.

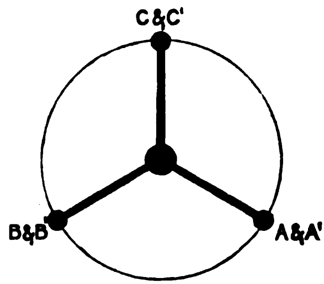


Fig. 12.

proposed, in letters which crossed each other, both by the author and by Mons. Mazen, of the Compagnie des Chemins de Fer de l'Ouest, who is interested on the part of that company in the development of the Heilmann system. There are six cranks, virtually forming two groups, each representing a three-crank engine, with cranks 120° apart, and therefore free from any tendency to vertical movement: if the two halves of the engine are thus free, the whole must be so. Each half of the engine, however, has a "couple," and tries to rock endways. It is designed to neutralise this effect by arranging the cranks as in Fig. 12—that

Mr. Robinson. is to say, by inverting the two groups relatively to each other, so that they shall rock systematically towards and away from each other. Both groups being mounted on one base, and enclosed in one crank-chamber, stiff enough to prevent deformation, it is clear that the engine as a whole will be subject to no rocking effect, and as it is also free from any unbalanced vertical forces due to the finite length of the connecting-rods, it is believed that complete success will be attained. A dynamo is mounted at each end of the engine, so that each half of the crank-shaft has only to transmit its own half of the power. It is evident that the two centre cranks, C and C^1, might be replaced by one, their two lines of parts being also replaced by one of the same total weight; the engine would then be five-crank, but it would lose certain advantages of interchangeableness of parts, and of manufacturing facility.

45. Two such double engines are now under construction for a large London station, but inasmuch as longitudinal stiffness is amply provided by the concrete foundation, it is unnecessary to place the two groups of cranks in one crank-chamber. Each set consists of two I.I.I.-S engines, each of 360 I.H.P., and each engine coupled to a dynamo. The engines are to stand end to end, the bases being bolted together, and the crank-shafts connected by a detachable coupling (the governors are driven by gearing, so as not to interfere with the coupling). The cranks are arranged as in Fig. 12, and the coupling does not admit of their going together in any but the right position. Either engine may be used, for light loads, as a single unit up to 360 I.H.P., in a form little likely to give rise to vibration, while at full power, when under ordinary circumstances vibration might become more serious, it is hoped that by coupling together as explained, it will be entirely got rid of. The more conclusive experiment, however, will be in the Heilmann locomotives, where the engines, instead of being on solid foundations, will be suspended on springs. It is not, of course, intended to imply that under ordinary circumstances the use of six-crank engines is necessary; the three-crank engine, on a good concrete foundation, is perfectly satisfactory in practice. But it is well to be prepared for those few cases in which the extremest precautions are required, either

because good foundations cannot be provided, or because the special nature of the sub-soil renders them less effective than usual. Mr. Robinson.

46. Perhaps the very simplest form of completely-balanced engine is one which was used in the earliest Heilmann locomotive. It is shown in Fig. 13, which represents a compound engine. Fig. 14 shows a variation which might be used for a triple-

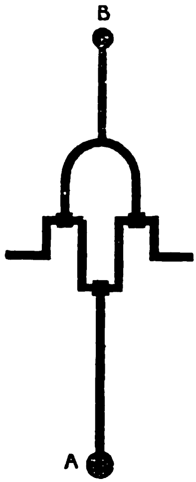


FIG. 13.

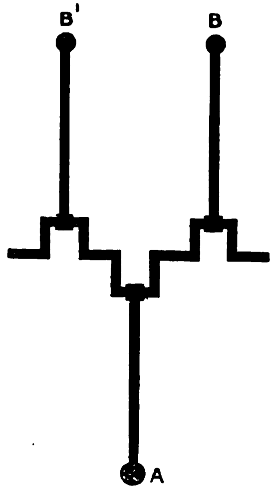


FIG. 14.

expansion engine. Of course, in both cases the total weight of the moving parts must be the same on the two sides of the crank-shaft, and the cranks must be 180° apart, and the connecting rods and cranks of equal lengths. Both figures are really a development of Fig. 9, but they provide, as Fig. 9 does not, for the varying inertia forces due to the finite connecting-rods. The forces vary at the same times and in opposite directions, and so neutralise each other completely, instead of only partially, as in Fig. 9. This form of engine needs to be horizontal. If used as a vertical engine, one crank must be driven from below, which is practically impossible in a marine engine, and cannot be generally convenient in other situations. Since the cranks must be opposite, such an engine also cannot have the even turning moment of an ordinary three-throw engine, and for marine

Mr. Robinson. purposes it would require to be duplicated (*i.e.*, to have six cranks) for the sake of avoiding dead centres.

47. Numerous proposals have been made to balance engines by "bob-weights"—dummy pistons driven by eccentrics. The couples in an engine can be thus balanced: not the other forces, unless the bob-weights are placed below, as in Figs. 13 and 14, and are as heavy as the pistons. The subject is, however, too complicated to receive more than superficial notice here.

---

48. In addition to the foregoing matters, it might be of interest to say something about the governing of central-station engines. This subject, however, needs a paper, to say the least, to itself: moreover, experience with single-acting engines can teach no lesson substantially different from what may be learned from double-acting engines, unless it be that the former, by reason of high speed and many impulses per minute, and the small amount of steam contained within the engine at any one time, are somewhat easier to govern. As regards steady running, in the sense of smallness of variation in speed during each revolution, the high-speed engine has, of course, enormous advantage. The engine before referred to as driving a flax mill in Belfast gives an almost perfectly straight line upon the recorder, although the instrument used is so delicate that it will show the change of speed during each revolution of even a high-speed engine. In this engine, with three cranks, there were 900 impulses per minute, all exactly equal, and it was not wonderful that with a pulley fly-wheel weighing only 6 tons, and of 5 feet diameter, better results were attained than in slow-running engines with fly-wheels several times as heavy. The same remarkably steady drive has been observed in another engine of the same type (but with two cranks only) driving a cotton mill. This, of course, is not evidence of good governing, so much as testimony to fly-wheel efficiency. The latter subject is, nevertheless, of great importance, and it is one which perhaps was somewhat neglected in the earlier days of the high-speed engine, when fly-wheel power was certainly cut fine.

49. As the fly-wheels have been made more powerful, so have

the governors. If central-station experience tells anything, it is that if you want prompt and sensitive governing and freedom from hunting, you must first provide a very powerful governor, and then give it next to nothing to do. The Willans governors are now, owing to the high speed of rotation, and the much heavier balls in use, enormously powerful in proportion to the work to be done, which consists only in moving the (piston) throttle-valve, and, in automatic expansion engines, the very small steam valve which controls the relay cylinder. The consequence is that a very restricted movement of the balls makes the desired change in minimum time, while leaving plenty of margin to deal with unaccustomed resistance, due to such causes as dirt or neglect. It is hardly possible that the most powerful shaft governor can have as much power, relatively to the calls upon it in shifting eccentrics, links, &c., as is possessed by the governor of any recent Willans engine. Mr. Robinson.

50. There is no subject about which more confusion exists in engine-rooms than the relative parts played by the governor and by the fly-wheel. They are certainly closely allied, but very often when the governor is blamed, it is really the fly-wheel which is in fault—that is to say, with reference to the particular work the engine is called upon to do. On the other hand, many a governor owes its reputation to the fly-wheel, whose huge weight forbids the engine to take serious notice of a change of load, until the governor, possibly in itself neither a powerful one nor a good one, has had time to adapt itself to changed conditions. A heavy fly-wheel cannot directly affect the permanent change of speed due to a change of load, but it depends largely upon the fly-wheel whether the momentary variation shall be great or not. It also favours accurate governing, by enabling a more sensitive governor to be used—such as, with a light fly-wheel, might set up hunting. With a heavy fly-wheel it cannot so readily do this; thus the advantages of a very sensitive governor may be enjoyed, without so much danger of suffering from the corresponding drawbacks.

51. The question of throttling *versus* variable expansion has been fully dealt with by Captain Sankey, in a paper read at the

Mr. Robinson.

Spring meeting this year of the Institution of Mechanical Engineers, though without particular reference to single-acting engines. It must suffice to say here that variable expansion is desirable for practical convenience, rather than for economy, in a great many cases, and for economy in some, but that in no ordinary case does the economy rise beyond a moderate percentage, and then only through a small part of the range of variation of load. It is also established, upon the basis of Mr. Willans's experiments, that at very light loads (say quarter load, or even more) it is distinctly more economical to throttle the steam than to make the cut-off still earlier, and thus to maintain the full pressure in the steam chest—since in the latter case, for only a small useful load, the full initial condensation is maintained, and the full loss by radiation. When the initial temperature in the first cylinder is reduced (as it is by throttling) both these sources of loss are diminished. Perhaps the most useful effect of automatic gear is that, by its aid, an engine can be occasionally overloaded, without having to carry the same late cut-off at all times. Where the normal load is also the maximum load (as it should be in lighting stations, where the plant works in parallel) the engine should be, of course, designed to give its best economy at maximum load, *i.e.*, with cylinders of such size as to give maximum load at only a moderate mean pressure. In such a case, when light loads are carried (if they ever are carried: it would be better to avoid them), it is not of much importance whether the power is reduced by throttling or by altering the expansion. At the other end of the scale is the case where the average load is small, perhaps only half or two-thirds the maximum; engines for electrical traction are a good example. Here it is evident that the cylinders, and the normal expansion, should be designed to suit the average load; but, since this load is small, automatic expansion must be used, in order to make provision for the occasional overload. By automatic expansion it is possible to keep the cylinders of such an engine small, and suitable for the light normal load, and yet be able to readily obtain more power from it for short periods. The periods of overload are, of

course, wasteful, but this is far less important than wastefulness during the much longer periods of average load. Mr. Robinson.

52. The mode of varying the cut-off in the Willans engine is by a sleeve working upon the trunk or hollow piston-rod, where it passes into the steam chest at the top (see description in Appendix A). There are inclined ports in the trunk and in the sleeve, and by rotating the latter by means of gear outside (or sometimes inside) the steam chest, the cut-off is made early or late. The system adopted is one originally planned by the author, but it owes its practical form to improvements carried out by Captain Sankey. A throttle-valve is used, which on a drop in load reduces the pressure in the steam chest in the usual way. The rotating sleeves are controlled by a small steam cylinder, the piston of which is exposed to boiler pressure on one side, and to steam-chest pressure on the other. On the drop referred to taking place, and a consequent fall in the steam-chest pressure, the piston moves towards the steam chest, and the cut-off is made earlier; means are provided to prevent too rapid movement of the cut-off gear in this direction, but slowness or jerkiness of action is immaterial, because the throttle-valve has always control over the speed. As the cut-off changes, the pressure in the steam chest gradually returns to what it was before, or nearly so. The journey of the relay piston in the reverse direction offers more difficulty, and if effected only with the apparatus as above described, would require a considerable permanent difference of pressure between the steam chest and the boiler. Captain Sankey has made this unnecessary by providing that when the engine speed *falls*, the spindle which opens the throttle-valve shall also move a small valve, which both cuts off the boiler steam from the relay cylinder, and allows the steam already in this cylinder to exhaust into the receiver, or to some other place of lower pressure; the relay piston consequently moves away from the steam chest, and makes the cut-off later. The point of cut-off is at all times indicated externally by an index and pointer, the latter forming also a handle to enable the gear to be worked by hand if preferred. The gear has the great advantage that by merely fixing a stop, the range of variation of cut-off can be limited,

Mr. Robinson.

on the one hand so that the engine cannot be grossly overloaded, and on the other so that the expansion may not be carried too far for economy. In fact, the engine governs by varying the expansion down to a certain mean pressure (dependent, of course, upon boiler pressure and other circumstances), *and below that by throttling*, which is exactly the ideal arrangement for economy.

In conclusion, the author thinks it may fairly be claimed that the single-acting high-speed engine has justified the favour shown to it by Electrical Engineers, in regard both to that which it has already accomplished and to the prospect which it holds out of further successful development. Its success, as in all similar cases, has fostered improvement in other engines, and it may be doubted whether central-station engines in this country, either of the slow-speed class or of the high-speed double-acting class, or even the steam turbine, would have attained to their present perfection had they lacked the spur applied by the progress of the Willans engine, and of others like it. For good or ill, the wide adoption of the single-acting high-speed engine marks a bold step in engineering. It is essentially of English origin and development, and its success bears witness that English engineers, though cautious, are also bold, when boldness is called for. That it has fallen to members of this Institution to be leaders in so striking an innovation, may perhaps earn a friendly tolerance for the many shortcomings of this attempt to place on record the more salient features of the system.

APPENDIX A.

Description of the Willans Central-Valve Engine.

The engine is single-acting, and on the "constant-thrust" principle, —that is to say, the connecting-rods are always in compression; never in tension. All the brasses are held close together, both upon the up-stroke and upon the down-stroke. The latter is the working or effective stroke: during the first portion of it the steam pressure on the pistons of course presses all parts against the cranks, and the crank-shaft against the lower main-bearing brasses; in the later portion of

the down-stroke the inertia of the parts, as they are brought to rest by the changing angle of the crank, also helps to keep them in compression. On the up-stroke, when no work is being done, and when the steam is being merely exhausted from the several cylinders, the inertia of the parts as they undergo acceleration by the crank also keeps them in compression for the earlier portion of the stroke; in the later portion they are cushioned by the arrangement to be afterwards described. The result is that in no part of the stroke, up or down, do the parts tend to separate; consequently there is total absence of "back lash" or "lost "motion," and "knock" cannot arise, as it would if the engine were double-acting, particularly in view of the high speed adopted. Mr. Robinson.

Fig. 15 shows a triple-expansion condensing engine in section (through one line of cylinders). The steam is distributed throughout by the hollow piston-rod R (sometimes called the "trunk"). It enters from the steam-chest by the oblique cut-off ports, shown near the top of the piston-rod. By the movement of the line of piston valves, which work inside the piston-rod (driven by the eccentric shown), the steam passes into the H.P. cylinder, at the beginning of the stroke, by the holes or ports shown just above the H.P. piston. It is important to remember that this ring of ports is the only inlet to and outlet from the cylinder, and that it moves up and down with the piston. The action of the valve inside is shown in detail in Fig. 1, which represents it in the exhaust position.

The valve gives just the same steam distribution as an ordinary slide valve, with a slow cut-off at about three-quarter stroke, or a little later. The actual cut-off is, however, effected by the oblique ports in the steam chest, which, at a point in the stroke either pre-arranged in the design of the engine, or controlled by the governor, pass behind rings, or a sleeve, so disposed as to prevent the further supply of steam for that revolution. As the cut-off motion is the same as that of the pistons themselves, the cut-off is very prompt, and shows a sharp corner in the diagram.

After the steam has worked expansively in the H.P. cylinder, the valve passes above the ports (see Fig. 1), and opens communication from the working end of the cylinder—*i.e.*, the space above the piston—to the space below it, which is called the H.P. receiver, but which is equally a steam chest for the I.P. or intermediate cylinder. During the up-stroke the steam is simply transferred from one side of the piston to the other; the whole cylinder, including the "working end," at that time forms part of the receiver.

When the next down-stroke commences, the steam in the H.P. receiver is passed into the I.P. cylinder. It enters the hollow piston-

Mr Robinson. rod again from the receiver by the ring of short square-headed holes shown, and passes from the piston-rod to the cylinder by the ring of ports shown just above the I.P. piston. Cut-off in this case is given by the square-headed ports passing into the gland in the I.P. cylinder-cover, and so losing the supply of steam from the H.P. receiver (=I.P. steam-chest). The cycle is exactly the same as already described for the H.P. cylinder, and at the end of the *second* revolution the steam fills the I.P. receiver. Thence, in the *third* revolution, it passes into the L.P. cylinder; and in the second, or exhaust, half of that revolution, it passes from the L.P. cylinder—*i.e.*, from its upper end to the lower end, of course without material change of volume or pressure. It is only during the first half of the *fourth* revolution that it finally passes away from the "transfer chamber" to the "exhaust chamber," the latter being in permanent communication with the condenser.

The full cycle thus described is that of a *triple-expansion Cornish engine*. In each separate stage of the expansion the complete Cornish cycle can be traced, and it is evidenced by two separate diagrams from the upper and lower end of each cylinder. The diagram from each receiver, or from the "transfer chamber," represents (as in every Cornish engine) the removal of back-pressure on the down-stroke, forming a virtual addition to the diagram from the upper end of the cylinder. The advantage is that the total range of temperature due to each stage of expansion does not take effect in the working cylinder, but only part of it. Part of the temperature range acts in the receiver only, where it does little harm; the limitation of the temperature range in the working cylinder is, of course, a clear gain in the matter of initial condensation. The adoption of the Cornish cycle in this engine is fully considered in the paper in paragraph 16, *et seq.*

The construction of the pistons, and of the rings, which are very distinctive, is shown in Figs. 1 and 2, and is fully described in paragraph 12.

A complete line of valves (from a compound engine) is shown in Fig. 3.

The arrangements for self-drainage from the cylinders are of great importance in the economy of the engine: they are explained fully in paragraphs 14 and 15.

All these points—viz., the special piston (and valve and gland) rings; the special arrangements for drainage; and the use of the "Cornish cycle" in all the stages of expansion—are of great importance, and are peculiar to the Willans engine. They are described at length, and their advantages are pointed out, in the paper, from

paragraph 11 onwards. (Steam-packed rings can be applied in double-acting engines, but only by providing two complete sets of piston rings, with follower, &c., for each piston.) Mr. Robinson.

The special glands (taking the place of stuffing boxes) are described and illustrated in paragraph 12.

The cushioning arrangement, before referred to, is special to the Willans engine. The guides take the form of bored cylinders, and the cross-heads are pistons, without rings. The top of the guide cylinder is closed, and on the up-stroke the air in it is compressed to the extent necessary to cushion the pistons and other parts. The power stored in the air by compression on the up-stroke is given out again during the immediately succeeding down-stroke, without sensible loss.

In a compound engine there are but two cylinders in series, but the "cycle" in each is precisely the same; in a simple engine only one. Sometimes the final "transfer chamber" is omitted (see paragraph 18), and the steam exhausts directly from the working (upper) end of the L.P. cylinder into the condenser or atmosphere—*i.e.*, the lower end of the cylinder is in permanent connection with the exhaust.

The line of valves is driven by an eccentric *on the crank-pin.* It is necessary that the source of motion for the valves should itself move up and down with the pistons, since the ports which have to be opened and closed also move up and down. There is an eccentric rod, which takes on to a hardened pin in a valve guide-piston; the latter works in a bored guide, formed inside the main guide-piston.

There are two connecting-rods to each line of pistons, one on each side of the eccentric: the eccentric rod plays between them.

The cranks and all working parts, except the cylinders and valves, are lubricated by the splash of the cranks in the crank-chamber, where the lubricant usually consists of a mixture of oil and water. The guides and the pins at the upper ends of the connecting-rods and eccentric rods are readily reached by the splash.

The cut-off in the first or H.P. cylinder is effected, as before stated, by the movement of the ports in that part of the hollow piston-rod which projects into the steam chest. Where the engine has an invariable expansion, the cut-off ports, as they are called, are of the same shape as those for the succeeding cylinders, and they lose their supply of steam by merely passing into the gland which separates the H.P. cylinder from the steam chest. If early cut-off is desired, the gland itself is raised a little by packing pieces between it and the cylinder top. In that case the ports enter the gland earlier, and cut-off is earlier.

Where the expansion is to be made variable, either by hand or by

Mr. Robinson.

the governor, inclined ports are used, and the upper end of the piston-rod is surrounded by a sleeve, which also has inclined ports in it. The sleeve is capable of a certain amount of rotation, which causes the ports in the rod to be covered (by the solid part of the sleeve) earlier or later. In the later patterns the sleeve is suspended by a spindle which passes through a gland in the top of the steam-chest, and the spindle is rotated by suitable links, &c. These are moved by hand, or by a relay cylinder, the action of which is controlled by the governor. The nature of the gear is more fully described in paragraph 52 of the paper.

It will be noticed, in Fig. 15, that there is a hollow space in the casting under the bottom of each receiver. This is supplied with steam, and acts as a jacket, passing heat into the upper part of the working cylinder, and serving to re-evaporate, in the receiver, the water which may enter it from the preceding cylinder. The later engines are without this jacket arrangement. Like all attempts to improve the Willans engine by jacketing, it has been unsuccessful. It reduces the consumption of steam in the cylinders per I.H.P., but the weight of steam condensed in the jackets just equals that which is saved in the cylinders. Apparently, a high-speed engine, *with good natural drainage* and ample sub-division of the total range of temperature, suffers too little from initial condensation to be capable of improvement by jacketing.

The President.

The PRESIDENT: Gentlemen,—I need hardly rise to propose a vote of thanks to Mr. Robinson for his paper, for, by your applause, you have practically accorded them to him. In rising to commence the discussion, I need scarcely call your attention to the extreme importance—the paramount importance—which this question of the development of the high-speed engine has been to us who are electrical engineers and power generators on a large scale. I think that our great progress dates from the time when the late Mr. Willans began to pay such attention to a high-speed engine, and I think that fact in itself is sufficient warrant for the paper that has been read this evening; and I do hope, after Mr. Robinson has acted in such a self-denying manner by cutting his paper up to a mere abstract while reading it, in order to occupy as short a period as possible, and so leave more time for those who desire to criticise and discuss it, that the discussion will be worthy of the paper, and that we shall be able to spend the remainder of this evening and the whole of the only remaining

meeting of the session on this important subject. I hope it is thoroughly understood that we invite gentlemen to speak upon the paper who, although they may not be members of the Institution, yet take a deep interest in the subject of which it treats, and that their remarks will be as welcome to us as those of our own members. The President.

Mr. RAWORTH: We have had a very great treat to-night in hearing a paper on a mechanical subject from one who is evidently not only a good engineer, but also a master of the literary art. Some of us have not always paid quite enough attention to this matter of finishing off our papers; but Mr. Robinson has not only chosen melodious words, but he has gathered them together in a harmony of composition. I quite agree with the sentiment, Sir, that you have just uttered, to the effect that the electrical industry owes a great debt to Mr. Willans for giving us this wonderful engine, which enabled electric lighting to be introduced in its early stages into the city and neighbourhood of London under conditions which probably no other engine would have so efficiently filled. I must say that I am a very great admirer of the enclosed engine; but I do not think that all the advantages which Mr. Robinson has claimed for the Willans engine arise entirely out of the fact that it is a single-acting engine. I do believe to a very great extent that those advantages accrue from the fact that it is an enclosed engine. There were many single-acting engines before the Willans engine, and even it goes back as far as the year 1874. At that time it was a very commonplace contrivance; it was simply a three-cylinder arrangement, in which the piston of one cylinder acted as valve for the neighbouring cylinder, and so forth. That engine was practically of little use, except to drive launches. It was, in fact, first cousin to the well-known Brotherhood engine, which in the early days of electric lighting had a great run for driving dynamos. I think Mr. Willans probably got his stimulus from that fact, and that he put his brains to work, to our very great advantage, and discovered the central-valve principle, whereby he made his engine not only quick running, but thoroughly economical. Mr. Raworth.

In his second paragraph Mr. Robinson says that a certain

Mr. Raworth.

paper—I take it, a professional paper—says that if we had stuck to the builders of mill engines and marine engines, instead of trying to strike out a new line of steam engines for ourselves, electric lighting would have been very much the gainer. I quite agree with Mr. Robinson that that is all nonsense. He says in a paragraph a little lower down that the total number of H.P. installed in electric stations in England is 101,390; and of these their firm, I understand, has made 53,000. Now the Brush Company have built 31,000, and there are only about 17,000 left. I do not know whether either of us is exaggerating. The fact remains that we have not been greatly indebted to mill engines; yet I will undertake to say that there is no one in this room who has had the responsibility for designing central stations but has at one time or another tried the effect of consulting one of those engine builders aforesaid. My experience has been that in nearly every instance they have failed entirely to grasp the situation. They knew how to drive a mill perfectly well; but when they come to drive a dynamo, which responds to the slightest variation in turning moment, and which reproduces the fluctuations of the governor with absolute accuracy—there being none of the steadying influence of two or three hundred H.P., which is required to drive the shafting of a mill—they fail.

In connection with this, I think I might tell you for one moment that I had the privilege the other day, at Leicester, of meeting Mr. Hudson, the distinguished engineer of Messrs. Hick, Hargreaves, & Co., of Bolton. In talking over the engines with him, he said, "There is not a mill engine in Lancashire which "would do this job." But, Sir, we in this room know perfectly well that a wave of opinion is going over the country just now to the effect that high-speed engines are merely temporary expedients, and that, if you want something that is to be permanent and substantial, you must get a big horizontal slow-speed engine. Now, Sir, if that argument be correct—that the slow-speed horizontal engine is a permanent thing, is a thing for which a corporation may put down the funds of the ratepayers, and which they may expect to see there in goodness knows how many years after—then that argument, and many more of the same kind,

would apply even more forcibly to the good old beam engine. When I was an apprentice, the same battle that is now going on between the horizontal slow-speed engine and the vertical high-speed engine was being waged between the beam engine and the horizontal. Instead of the horizontal engine being the respectable, hoary-headed old rascal it is to-day, it was then a new-comer, it was the cheap thing, it was the article that you could put down without much foundation; but the engine which was pointed to as being a really practical, satisfactory, and lasting article was the beam engine. I have no hesitation whatever in saying that if the horizontal engine will last 40 years, the beam will last 80 years; and if any corporation thinks there is any desirability in saving a few pence or pounds in repairs per annum, and spending a great many more pounds in interest and sinking fund, then I should say, By all means put it in a beam engine. Mr. Raworth.

Going back to the Willans engine, I may say I am a very great admirer of the contrivance, and Mr. Robinson has put its features most admirably before us; but it has one defect—which I, being only a common engineer, consider to be rather a serious one—namely, that it is a Chinese puzzle; and we cannot afford to keep a Japanese always on the premises to work it out. When I say it is a Chinese puzzle, I mean that one cannot individually take out any of the parts in order to see whether that part is in order or not. In illustration of this, let me refer you to the picture which Mr. Robinson has supplied with his paper. You will see a central column, nearly as big as the Eiffel Tower, working on a very small connecting rod. The fault of the Willans engine consists in the fact that you have to take down the whole of the pillar in order to get at the bottom piston, or the bottom gland. Some years ago, I was trying to discuss the Willans engine with a very distinguished member of this Institution—a gentleman who has a very large number of these engines under his care—and he said, "Now, Raworth, I really do not know what there is "inside a Willans engine; but I know there are three holes in it— "one hole where the steam goes in, another where the steam "comes out, and another where you put the oil in; and if I only "pay proper attention to those holes, everything will go all right."

Mr. Haworth. The Willans engine never received a higher compliment than that!

But now I want to take you for a moment to the question of high speed. Mr. Robinson seems to think that the speed of revolution arises naturally from having the thrust entirely in one direction, and I find a great number of people who are carried away by the same specious argument. Now let me ask you for one moment to consider that the Willans engine, giving 360 I.H P., has only a stroke of 9 inches. Did anybody in the world ever see a double-acting engine with anything like so small a stroke for that large power? I am free to confess that, as far as my experience goes, there never was such an engine; and the reason is obvious: it is simply because, if you were to make a double-acting engine of the ordinary kind with so short a stroke, the port clearances would be so great that they would destroy the economy of the engine. But I distinctly affirm that the high speed of the Willans engine is not due to the constant thrust, but to the short stroke. I am quite willing to admit, however, as Mr. Robinson has said, that with constant thrust you do get the advantage of automatic take-up of wear; whereas with the double-acting engine, if you run it for a few months, you get an appreciable amount of wear and of slackness. I notice with considerable satisfaction that Mr. Robinson admits in the paper that for the extremest speeds one must use a double-acting engine. The reason is obvious, I think. If I interpret Mr. Robinson's mind correctly, it is because in the back stroke of a single-acting engine you have to put velocity into the working parts out of the crank, and not out of the steam, and the strain is apt to become very excessive.

I am quite ready to admit that the Willans engine is pretty free from wear, but I do not attribute it to constant thrust. I have had large experience with bearings running under very considerable weights, acting entirely in one direction, as in the Mordey-Victoria alternator, in which the magnet weighs a considerable number of tons. With the high speeds of the old belt-driving machines it was difficult to get them to run cool until we adopted forced lubrication. An engine bearing will run

perfectly well with 500 lbs. per square inch, yet similar bearings when applied to that same dynamo would not run with more than 70 lbs. to the inch. The reason was obvious. The alternation of stress on the engine bearing permits the oil to enter freely, and it gives an opportunity of getting better lubrication than with a journal sitting down under constant pressure in one direction. Mr. Raworth.

I wish to refer to one point which I have had out once before with either yourself, Sir, or Mr. Robinson, and that is, the question of brake efficiency. The Willans people have always claimed for their engine a higher brake efficiency, on the average, than they said could be obtained with ordinary engines. Now I claim that an ordinary vertical engine, if properly constructed, will run, not only with the same efficiency as the Willans engine, but with higher efficiency. We do not often make tests of these things, and we are not often in a position to say definitely what the brake efficiency is; but the engines for the City of London Electric Lighting Company were tested by Professor Kennedy, and the combined efficiency of the engine and dynamo together was 83 per cent. The dynamo does not exceed 90 per cent. efficiency. This leaves 93 per cent. for the engine, because it is only a common engine; it is not a Raworth engine, it has only an ordinary crank-shaft with ordinary pistons: this engine comes out at 93 per cent. efficiency. I mean to say that any ordinary Tangye engine—I am not saying that with a view of casting any reflection on the Tangye engine, but it is an engine which you can buy out of a shop window—any ordinary vertical engine with a balanced slide valve, will give 90 per cent. and upwards in brake efficiency. But with slow-running horizontal engines I quite admit we cannot get this high efficiency, because we have to drag backwards on a long railway, without wheels, a heavy piston which is only very imperfectly lubricated, and that is where the power is lost in a horizontal engine. In conclusion, I heartily concur in the sentiment that we electrical engineers are greatly indebted to Messrs. Willans & Robinson for the highly developed machines which they have put into our hands for overcoming the difficulties of many very awkward conditions.

Mr. W. H. Booth: The author refers in the paper to the over- Mr. Booth.

Mr. Booth. lapping of the temperature ranges in the cylinders of ordinary compound engines, stating that such is not the case with a Willans engine. Now I do not think that is altogether correct.

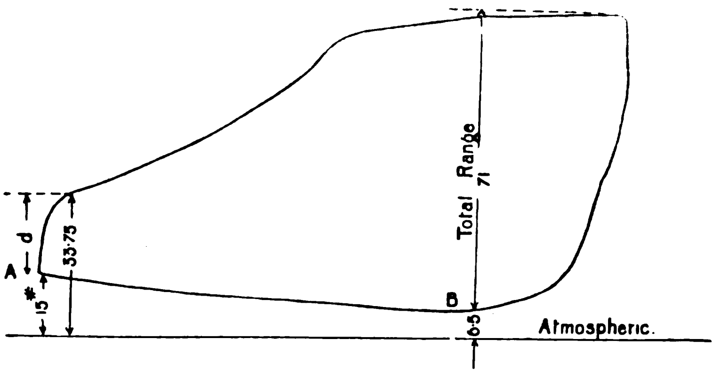


FIG. 1.

Fig. 1 is a very ordinary sort of a diagram, in which, at the exhaust, you get a drop from 33·75 to 15 lbs., and a total range in the H.P. cylinder of 71 lbs., or 8·5 lbs. more than there should be if the exhaust line did not slope downwards from A to B, thus allowing the pressure to fall below the initial possible pressure in the second cylinder, namely, 15 lbs.

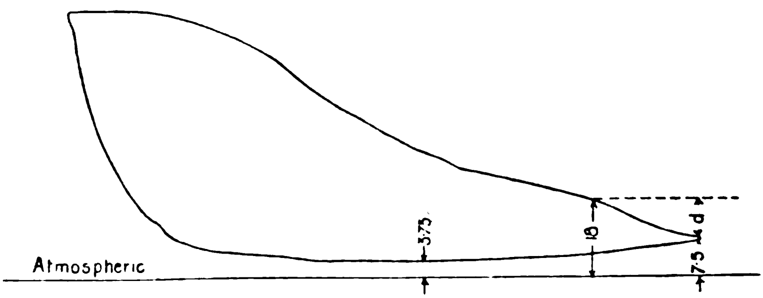


FIG. 2.

You get a similar effect in Fig. 2, but in this case only 3·75 lbs. greater range than there should be in the cylinder. If you close early the admission of the low-pressure cylinder, thereby bottling up the steam in the receiver or pipes, you may reduce the slope of the exhaust line as much as you desire, and may

diminish both the drop, *d*, of Fig. 1, and the overlapping of pressures in the two cylinders indicated by the back-sloping exhaust line. Mr. Booth.

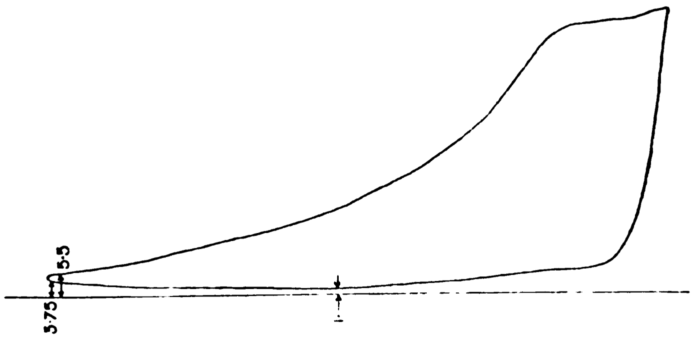


Fig. 3.

In this way Fig. 3 was obtained, the overlap being only 2·75 lbs. In this diagram the expansion curve continues into the exhaust line without much drop, and the exhaust line then continues nearly horizontal—in fact, almost like the theoretical diagram that Mr. Robinson has given us to-night.

The low-pressure diagram in this last engine is given here (Fig. 4). It would be a better example but that the engine was an

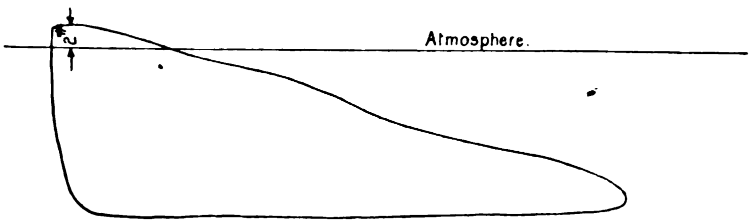


Fig. 4.

old beam engine McNaughted, and had therefore too small low-pressure ports for a compound engine. There need be no overlapping of the range of temperature of the two cylinders in the ordinary compound engine if properly designed and the valves properly set. I think that this is a point which ought to be insisted upon, and an ample receiver capacity should accompany this system of working. I admit the value of the Cornish cycle,

Mr. Booth. but I think ordinary compound engines get the same result as the Willans engine, except, of course, in the final cylinder.

Mr. TREMLETT CARTER: Is it a receiver engine?

Mr. BOOTH: Yes; it is a small receiver only.

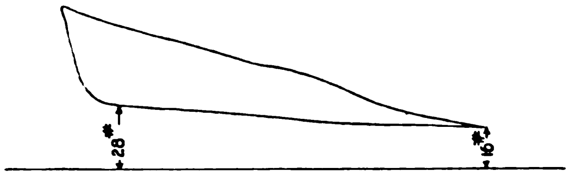


FIG. 5.

[*Communicated*].—I would now add Fig. 5—a recent diagram from the intermediate cylinder of a four-cylinder triple-expansion engine, for which I am indebted to the *Practical Engineer*, May 10th, 1895, and which illustrates the point still better. In this diagram there is no drop at all in the exhaust line, either before the end of the stroke or during the exhaust stroke. I would add that this system of working compound engines has been familiar to me for over 20 years, and, so far as I know, common knowledge before that time; but it is constantly being brought forward as new, or evidence is continually forthcoming, as in the paper, that it is not so well known as it should be. It is clear, however, that the Willans engine in all but the last cylinder has no advantage whatever over any other compound engine; for the range of temperature in the Willans must always be at least as great as that indicated in Diagram 5, than which no compound engine diagram need be inferior.

Mr. Carter. Mr. TREMLETT CARTER: May I make a few remarks in connection with this very same point—the overlapping of temperatures? The cylinder condensation really depends upon the temperature fluctuation of any given mass particle of the iron. Now what Mr. Robinson has shown is, that in the Cornish cycle the fluctuation of temperature is divided in two; but, as a matter of fact, the fluctuation of temperatnre in any one particle of iron in the cylinder is not thus divided, but is really continuous, just as if the whole expansion took place in one stage. Let us consider any portion of the cylinder liner: that portion when it is on

one side of the piston undergoes the fluctuation of temperature represented by the upper portion of Fig. 6; and after it is passed over—after the piston has moved forward so that this particle of the liner comes on to the other side of the piston, and gets into action on the other side of the cylinder—then it undergoes the fluctuation of temperature represented by the lower portion of the diagram. My point is this—that the two stages in the fluctuation of temperature do not take place in two separate cylinders and two separate surfaces, but take place in one cylinder and with only one surface, and every particle of iron undergoes two stages of temperature. Therefore, penetration of the fluctuation of temperature, which causes cylinder condensation—the variation of temperature of the iron, which causes the water to be condensed on the surface on the admission of further steam—is just the same whether we have a Cornish cycle, or whether we have an ordinary expansion in one stage during one stroke. Mr. Carter.

Colonel M. T. SALE, C.M.G.: There is one point which I should like to bring before your notice, and that is, the relative cost of these methods of driving. It so happens that recently there was occasion to call for tenders on behalf of the Government for a certain engine with a dynamo directly driven to turn out 500 E.H.P. The choice lay between the slow-speed Corliss type engine with a fly-wheel armature, a comparatively quick-speed vertical engine driving a multipolar dynamo of somewhat large dimensions, and, lastly, a high-speed engine driving a comparatively small dynamo at a comparatively high speed. The speed of the low-speed engine was 80; the speed of the vertical engine and dynamo was 120; the speed of the high-speed engine was not specified. The results as regards price were curious. One would naturally suppose that the price would be least for the high-speed engine and high-speed dynamo, both being comparatively small, but that was far from being the case. The highest prices quoted (but not much the highest prices) were for the slow-speed engine with fly-wheel armature; next in order of price came the high-speed engine with the high-speed dynamo; and last, and by far the lowest priced of all, came the vertical engine and the multipolar dynamo to run at 120. Those Colonel Sale.

Colonel Sale.

facts are, perhaps, unexpected, at anyrate by myself, and perhaps may be unexpected by the gentlemen here, but they are facts. I should add that all the firms whose tenders were considered were of high standing and reputation.

The discussion was then adjourned.

The PRESIDENT announced that as the result of the ballot the following candidates were duly elected:—

Foreign Member:

Don Rafael Casado.

Member:

Stanley Bright.

Associates:

Cornelius Francis Fielding.
Adam Rutherford.
Rigby Wason.

Student:

John Harold Wray.

The Two Hundred and Eightieth Ordinary General Meeting of the Institution was held at the Society of Arts, John Street, Adelphi, on Thursday evening, May 23rd, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on May 9th were read and approved.

The names of new candidates for election into the Institution were announced, and, this being the last meeting of the session, it was agreed that the candidates should be balloted for that evening.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Edward Tremlett Carter.

From the class of Students to that of Associates—

John Richard Jesse Bowden.	Guy Percival Morrish.
Arthur Frederick Harris.	Carl Adolf Louis Prüsmann.
Frank Holmes.	Leonard Leslie Robinson.
Owen D. Lucas.	H. G. Solomon.

Mr. W. R. Cooper and Mr. P. B. Crowe were appointed scrutineers for the ballot for new members.

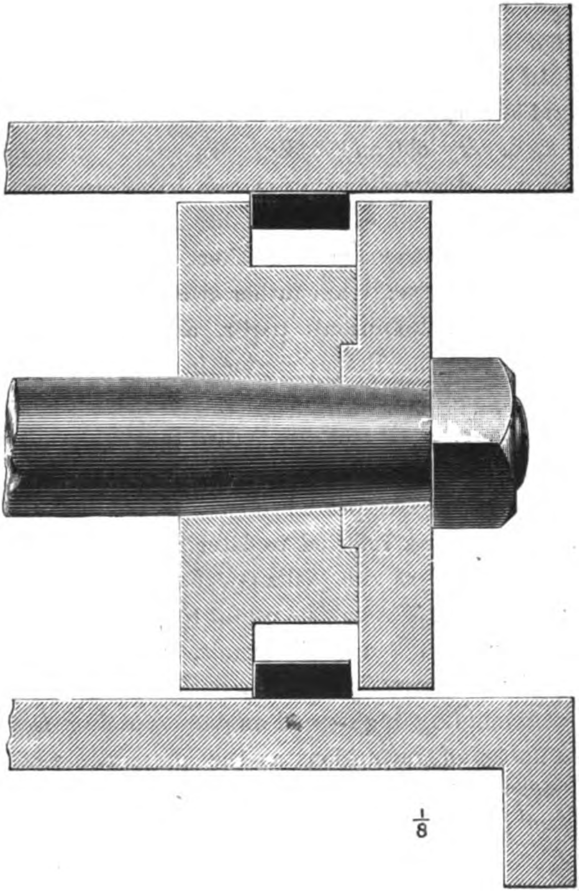
Donations to the Library were announced as having been received since the last meeting from Messrs. Baillière & Fils; Dr. F. Bedell; Mr. H. Wilde; the Director-General of Telegraphs (India); Mr. A. Blondel, Foreign Member; Mr. W. Perren Maycock and Professor R. M. Walmsley, Members; to all of whom, on the motion of the President, a vote of thanks was unanimously accorded.

The PRESIDENT: We are now in a position to resume the discussion on Mr. Mark Robinson's paper, "On the Recent "Development of Single-Acting High-Speed Engines for Central "Station Work."

Mr. Siemens. Mr. A. SIEMENS: I think it is a singular coincidence that on this day, the anniversary of Mr. Willans's death, we are discussing his engine. I should not occupy your time at all if Mr. Robinson had not mentioned my Newcastle paper, and the figures given there. I would draw your attention again to this—that they were not test figures for show trials, but they were the results obtained in the regular work of the central station at our works at Woolwich; and with regard to the results obtained by the Belliss engine, I would specially observe that that engine was the very first of its kind which had been made by Messrs. Belliss, and that it is natural that later engines would show better results than the one about which I spoke at Newcastle.

Mr. Chandler. Mr. N. CHANDLER: I have a few remarks to make upon certain matters of detail, having been kindly invited by the President and Council to do so. There are one or two points in Mr. Robinson's happy paper with which I cannot quite agree. First, he states that the slide valve rings are not "steam packed." Now, if they are not steam packed, what prevents them from collapsing during their transit past the port-holes in the "trunk"? It would appear pretty certain that there must be considerable pressure inside of the rings, or else they would collapse and leakage would take place, and reduce the splendid economy of the engine. It would be most interesting to all advocates of piston valves to know what really are the conditions of pressure at the back of the valve rings. Then, again, he tells us that steam is admitted to the space behind the piston rings, and this steam, assisted by the thin junk ring, or "follower," keeps the rings in constant thrust downwards. This, of course, is true; but the same conditions are arrived at with the old-fashioned piston which is illustrated on the blackboard. We will suppose this piston to be single-acting. Seeing that this old piston ring has steam behind and on top of it, this ring should not wear itself or the cylinder walls

any more than is the case with the Willans or any other single- Mr. Chandler.
acting engine having the same steam cycle. The cause of longevity of the cylinders and rings in single-acting engines is owing to the fact that only through the down stroke are the rings exerting considerable pressure on the cylinder walls.



Therefore wear is taking place during only half of the revolution of the crank. Thus, if the piston speed happens to be 500 feet, wear is taking place only over 250 feet of it; and this applies to the engine with which my name is connected, and which has precisely the same steam cycle as the Willans engine. Referring

Mr. Chandler. to the sketch on the board, the white spaces at the back of the old piston ring represent steam space, and the piston is supposed to be very much worn or a very loose fit in the cylinder; and therefore steam is on the top of the piston ring and at the back of it, forcing it outwards and downward, thus causing a comparatively steam-tight joint. During the up stroke the piston flange would force the ring upward; and when it gets near the top, centre compression would still exert a downward pressure until the new steam entered and forced it still harder outwards, and downwards on to the bottom flange of the piston. I have known a very old and badly worn engine working under these conditions. I simply mention this to show that not only the most modern pistons of the day, but also the most ancient, sometimes work under the same conditions.

Mr. Morcom. Mr. ALFRED MORCOM: I am rather at a loss as to the manner of my reply to Mr. Robinson's paper, as he has referred so extremely politely to the Belliss engine in three or four points. But it has been pointed out to me that the whole tenor of the paper was to condemn it quite as much as any of the other engines to which Mr. Robinson has been referring. Now I may say at once that we have proved conclusively that in all the points Mr. Robinson has referred to later on, we have an engine that is at least as good as the Willans. I shall prove my words further on; but I think it will be useful to deal with the matter historically, as far as my time will permit, in the same way that Mr. Robinson has done, and show why our engine was designed. We have been building high-speed engines, running up to quite as high a number of revolutions as the present electric light engines—I suppose 500 or thereabouts—for all kinds of powers for a matter of nearly 30 years, and we knew what the double-acting could do and what it could not do. When we were brought face to face with the necessity of devising an engine that would run continuously for station work at the higher speed, we naturally looked at the single-acting engine as a possible means out of our difficulty. We were building them for the Navy, and, of course, our first view was in that direction. The things we saw there, as far as the single-acting engine went, were

certainly discouraging. Now I do not want to say one word that will in any way annoy Mr. Robinson; I shall merely refer to facts, and his reply to them will be, I have no doubt, helping his view. The engines for electrical purposes in the Navy some years ago were nearly altogether single-acting engines. The Admiralty latterly, owing to the early difficulties that occurred with single-acting engines, permitted double-acting engines to be used at the same time, but they would not permit them to run at more than about three-fourths the number of revolutions—a very serious handicap in price. Gradually reports came in from one station after another as to serious collapses of the Willans engine—lines of valves coming down, crank-shafts breaking, pistons smashing altogether; in fact, I may say—I cannot answer for this; I have had it from the officer in charge of one of the foreign yards—that they were not able to repair Her Majesty's ships in a proper way because the Willans engine took up so much of their time. That may be exaggerated, but the fact remains that at the present time the Admiralty—and not only the Admiralty, but a number of other Governments—will not have a single-acting engine on board ship to drive the electric light, because of the disasters that have occurred. But the double-acting engines have been going through just the same duty alongside of them, and they have not shown these collapses or difficulties. We started with that before us, and we had to consider what our double-acting engine was doing without forced lubrication, and what it might do with a system of good lubrication; and we finally made the arrangement as we now have it—that is, pumping the oil to every bearing of the engine, keeping it there under pressure, and letting it work out its benefit in its own way. The benefits derivable are, in the first place, durability; and, in the second place, and largely, economy. The points we looked at were these: Taking the single-acting engine as it stood, as compared with the double-acting engine, there was always the fact that you had twice the area of piston, which means double the strain along each rod, double the pressure on every bearing, and, therefore, under ordinary circumstances, double the wear. Mr. Robinson has

Mr Morcom.

Mr. Mergom. referred to the constant thrust as being such a favourable feature in the engine. This constant thrust is a thing that I many years ago had some experience with in the oil-testing machinery at one of the dockyards. There we had an arrangement made with a half brass bearing on a journal, which was running underneath it, and the oil to be tested was let in through the upper part of it. We had a dead weight put on to give pressure, so that the fitting was, as in the Willans engine, a dead thrust on the bearing. Marine engineers knew long ago that the thrust of an engine requires to be specially looked after, because there is so little easement to permit the oil to enter. But in this oil-testing arrangement we could never get the thing right until we fitted a small arrangement to jig it up and down—making it, in fact, like a double-acting engine—and then it would work well with a tenth part of the oil. Those were facts that certainly helped us along to decide this design of double-acting engine, knowing that we were going to come somewhere near, if not up to, the Willans. We designed this engine with the foregoing facts in view, and it is there, for good or bad, to be compared with Messrs. Willans's engine. To make the comparison clear I will just take up the points that Mr. Robinson has referred to one by one, and see how we have fared. First, as to economy.

BELLISS-CROMPTON COMBINATION FOR ST. PANCRAS.

Result of Six Hours' Water Consumption.

Hour.	Steam Pressure.	I.H.P.			E.H.P.	Water per			E.H.P./I.H.P. Efficiency.
		H.P. Cylinder.	L.P. Cylinder.	Total.		I.H.P.	E.H.P.	Hour.	
1	123·3	85·78	115·89	201·67	178·6	18·0	20·35	3,635	88·5
2	123	85	117·23	202·23	178·8	17·8	19·66	3,516	88·4
3	121·75	86·93	114·98	201·9	179·1	17·5	19·8	3,550	88·7
4	121·5	85·8	116·77	202·57	177·2	17·5	19·9	3,589	87·47
5	120·5	85·57	115·77	201·34	178·8	17·48	19·69	3,521	88·8
6	120·5	86·97	113·58	200·55	177·6	17·7	19·98	3,550	88·5
Mean	121·75	86·0	115·7	201·71	178·35	17·58	19·89	3,551·8	88·39

I have got here the figures of a trial of one of the sets of

engines for St. Pancras that have been recently made under conditions very parallel to those that obtain at Thames Ditton; in fact, we have copied their testing arrangement as accurately as we know how to do it. The tests were made in the presence of Professor Robinson's assistants. There were four gentlemen, in fact, looking very keenly after them, and I have no doubt they are very accurate. There were three engines of the sort. The results are so similar as to leave no doubt that the figures are practically correct as they stand on the paper. You have there an engine certainly as good as we can make it at the present time, and it shows with a pressure of steam of about 120 lbs. that the water per E.H.P. has come out to a mean of 19·89 for an average of six hours, running under full power. The power we ran the engine at was about 10 per cent. higher than the maximum power required at the station. There is an extra full power required occasionally; but, as we had to set our slide valves to that, we preferred to run the engine at this power, to get the best result possible. The result is, I think, at least as good as the Willans engine can do with that pressure of steam. Taking the dynamo efficiency at 96, which the makers give it—and certainly from the magnificent way in which it ran it was well worth that efficiency—the efficiency of the engine is 92, the higher figure that Mr. Robinson spoke about. We have had numbers of other trials before different gentlemen giving similar results; but we have not been able at present to have the trials made in the same way as at Thames Ditton. I hope that when we have the new test house we are building completed we shall be able to show the results in a more complete way; but there is no doubt about this fact—that there is not a halfpenny difference between the two engines as far as economy goes; and you may take my word for it at present that it is no use paying any more for it. Mr. Mcreen

As far as durability goes, Mr. Siemens has referred to the engine that he has spoken about at Newcastle. That, of course, was one of our first engines. I wish it had been, considering the position it is in, one of our last. But I may say, in passing, as far as the economy goes there, our engines show up much better for the low loads than the Willans engine, which, however, appeared to

Mr. Morcom. be better for the high loads. We had a two-crank engine with cranks opposite, against a Willans-Robinson three-crank; but the vibration question came in, and we had, as a matter of experiment, to put on our exhaust, lap enough to bring on compression to steady the engine. The amount of exhaust lap that was on was so great that the diagram showed only about 4 lbs. and a decimal on the exhaust side of the low-pressure cylinder. The diagrams recently sent, since they have had the engines put right and the lap taken off, show 8 lbs. There is a matter of 3½ lbs., which, with other minor adjustments, would have brought the engine quite up to and beyond what the Willans-Robinson engine was for the full power, and we should also have more largely beat them at the small powers. The question, of course, there is dependent upon the serious amount of friction there is in the numerous rings of the Willans engine.

There is no use in comparing the parts of the engines as they existed in the trial made at Messrs. Siemens's works, but the parts of the Willans engines are far and away more numerous. The rings are, as Mr. Robinson has pointed out, greatly more numerous, and I believe he has put his finger on the reason why our engines showed up better at the low load. But that question of piston-ring friction is a big question, and it is going to be a much bigger one by-and-by. At the present time super-heating is largely in the air, and I think that I should be willing as an engineer to predict that in the next year or two super-heating will be very largely in electrical stations. When the super-heating comes in, the rings and the friction that they may have will be a very different thing to what they are now. The necessity for cylinder lubrication will also be of more importance there; and especially if they are going to put water-tube boilers in, there will be an absolute necessity to keep the lubrication out of the boilers. In fact, if condensing and super-heating come in, the Willans engine, with its numerous piston rings, and with its general capability of putting oil into the boilers, will stand the worst chance. I am, of course, saying all I know—and perhaps I do not know all—in favour of the double-acting engine as against Messrs. Willans's

engine in particular. As far as governing goes, one would say off-hand that a double-acting engine ought, having two impulses in the revolution, to be a better engine than a single-acting for governing; and I have seen some single-acting engines where it has been necessary to throw the governors out, and to govern the station by means of an engineman at the throttle. I think Mr. Robinson has pointed out the impossibility of a double-acting engine doing as well as a single-acting engine as far as the governing goes. We had not taken much trouble about them, because we have not had any difficulty in doing all that people have asked us. During the last three months, however, we have turned our attention to the point; and I think Mr. A. H. Preece can say, having seen some experiments at the works the other day with regard to governing, that they could not be excelled by any single-acting engine. It showed simply that the governing by throttle was isochronous when the load was thrown off from a full load to nothing all the way up. It did not vary more than a revolution or two, and the run up—which was the great point we were trying to improve—the sudden run up when the load was thrown off—only amounted to about 3 per cent. You do not want better governing than that, and it can always be repeated in this double-acting engine. As far as vibration goes, the last time I saw the diagram on the wall was in this same hall. Mr. Robinson brought his six-crank engine forward, not as a marine engine, but in connection with the subject of vibration. We attacked the problem put before us, of endeavouring to stop the vibration in an engine for a central London station, and I think we shall do it yet. But the engine we put in, although it entirely did away with the rocking moment, did not avoid the up and down thrust due to obliquity of connecting rods; and there is no doubt, as Mr. Robinson says, the up and down thrust is more important than the rocking moment—on concrete, at any rate

Mr. Morcom.

Fig. 14 shows very well the nature of the alteration we are going to make to the engine to endeavour to put it right—that is, to reverse the connecting rod, and, in that way, to get a perfect balance. The engine is coming back to us, to be replaced

Mr. Morcom. by Messrs. Willans's six-crank sets, I am sorry to say, and we are going to modify it and see what we can do; but I am perfectly sure it will give a much better result. If it does not, we have, as an alternative, at least a three-crank equal-angle engine; and there is no reason why a three-crank double-acting engine at equal angles should not be at least as good, as far as vibration goes, as any single-acting engine. There is nothing in the single-acting engine to give it a benefit there; but, on the contrary, the double area and consequent increase of moving weights will tell against it.

I have been obliged to speak out as I have done, in reply to Mr. Robinson's references, because we have been behind the scenes, and people do not know us. Messrs. Willans have been in the front. They have done excellent work; they have supplied a want when it was needed, and they are earning the result. If Mr. Robinson had left my engine alone here, I should not have been on my legs; but he has spoken, and I must answer. And I myself am perfectly satisfied with my answer. I feel that we are making progress, and I am content to let the engine rest on its merits, and make its progress only by its merits. With reference to my remarks about the Navy, I hope it will not be believed that they have been made altogether in bitterness of spirit. I have mentioned the fact because it is, in my mind, a very big one. There you have the engines put together in a place where they certainly will not get the attention they can in a central station, but the single-acting engines are getting the same attention as the others. They fail right and left, and are being pulled out on this account, to be replaced by double-acting ones. This seems to show conclusively that a double-acting engine, as far as design goes, is the simpler and better one. If we can, by means of forced lubrication, do what I have said, we get noiselessness such as theirs has, and we get durability at least as good as theirs. The engine at Messrs. Siemens's I saw myself opened up. It had been running two years. The bearings were not altered. There had been no wear, and no adjustment necessary. The engine which has just been taken out of the station I am referring to, on account of its vibration, was looked

at very critically and carefully for signs of wear, and I am Mr. Morcom. perfectly sure that the gentlemen who are in charge of the station are as impartial as it is possible to be—at any rate, they are not more partial to us than to the other firm whose six-crank engine is replacing our opposite-crank one on account of vibration. The gentleman who inspected the engine thought it his duty to send us a letter—he had previously expressed surprise at the absence of wear—to tell us how pleased he was with the way the engine had opened up. They had done nothing whatever to it since we left it in their hands, and it was in absolutely perfect condition when we took it back again—that is, as regards durability. The economy is shown by the table on the board, and further tests I hope at some time or other will be carried out to show still better results. I simply ask, Where is the quality in the single-acting Willans engine that should make it better? Ours has only been about three years in the market. It has been through its childish troubles. It has had the little defects due to new designs. Piston and piston rod, packings, and the shape and thickness of the casings had to be altered, and a number of other minor things, and the engine at present is in a far more perfect condition than it was. Although it is impracticable to challenge Messrs. Willans to have a public trial, I should be very glad indeed if one could be made on the lines that Mr. Siemens arranged, to determine the relative merits under the several heads referred to. That would serve to satisfy electrical engineers that they have another engine, at any rate, that can do them thoroughly good service.

The President. The PRESIDENT: I think it would preserve the proper fitness of debate if some large user of the Willans engine would now give his experience. Mr. Wright is a large user: perhaps he can tell us something.

Mr. Wright. Mr. ARTHUR WRIGHT: I am afraid my remarks will be very uninteresting. I have 2,000 H.P. of these engines under my control, and have had no bad experience at all. I can say nothing that is not complimentary, and compliments are hardly the subjects for a discussion that will be interesting to a meeting

Mr. Wright. like this. I have no fault to find with the engines, having never had a mishap with them. All the eight engines are attended to by two men and a lad. As to economy, the results show that we at Brighton can do as well as other people.

Mr. Halpin. Mr. DRUITT HALPIN: I will only refer to one or two points. At the last meeting we heard some very high figures indeed about brake efficiency. I have never had one of these single-acting engines on the brake, and I do not know about their efficiency. We heard a good deal about the common or garden engine running into what seemed to me to be fabulous efficiencies of 91 and 92, and I think 94 per cent. if I am not mistaken; and I certainly cannot believe in figures of that kind unless I am given the means of verifying the statements made. Of course, as we know, there is nothing that will lie more consistently than an indicator, except, naturally, a voltmeter or an ammeter. We also know that it is very easy to get very wrong indications with the brake. There was great trouble and scandal, as we all know, some years ago about the question of the brake, and it was shown that most brakes were all wrong, and all the results got from them were incorrect. I will tell you one thing that makes me doubt these high efficiencies. I saw an engine capable of braking 30 or 40 H.P. I saw that engine running empty, just turning round and well warmed by steam. She had been running then pretty freely; three powerful men, when the engine stopped, separately took hold of the fly-wheel and shoved it round, to see how far they could make it revolve, and one of them, either by superior strength or skill, after he let the wheel go (and we noted it carefully), got three revolutions out of it; so that there was not much wrong with that engine as far as friction went. She was about as free as an engine could be made. That engine, on careful braking and careful indicating, barely ran into 90. She did not go into 91, but only just barely into 90. With reference to what has been said about the oiling, and the necessity of having reversal of stress at the end of the stroke, with the change of stress to enable the engine to run, I remember, in discussing the same subject once with the late Mr. Belpaire, the

chief locomotive engineer of the Belgian State Railways, he said his view was that railway travelling would be absolutely impossible, merely from the point of view of the lubrication question, if it were not for the slight tremors produced by the rail allowing the lubrication to get in to the bearings; or that otherwise the whole thing would stop, and no efficient system of lubrication could be maintained. But I certainly cannot agree with the last speaker that it is necessary to have this perfect reversal of stress to get lubrication in, and I have seen and read Mr. Siemens's paper with very great interest, about the results of the double-acting engine; but I must say myself, for continuous high speed I do not see, even with the very elegant system of lubrication there introduced, how they are to keep it continuously and successfully running, in spite of all the good workmanship. Mr. Halpin.

The PRESIDENT: If there are any engineers present, we should very much like to hear them speak. They are the gentlemen whose experience we most wish to have on the matter. It is the gentlemen who work the engines whose opinions we want to hear. The President.

Mr. GEIPEL: The paper which Mr. Robinson has put before us bristles with information and points which are of the utmost interest to practical electrical engineers. There are one or two points in the paper, however, to which I will refer, with a view to obtaining even more information from the author. In the first place, in speaking of the costliness of slow-speed direct-driven steam dynamos, I take it Mr. Robinson refers only to first cost; otherwise, is it not somewhat previous to state that high-speed will last as long as slow-speed engines? We know that there are slow-speed engines which have been at work for 42 years and upwards which are running at the present day; that the Willans or any other high-speed engine will do this has yet to be proved. If they will not last so long, then it is necessary to take into consideration whether the earlier renewal of the one will balance the increased first cost of the other. Mr. Geipel.

We all know how seriously the cost of electric supply is increased by the lying idle of so large a fraction of the plant as is necessary to meet the too short periods of heavy load; and if

Mr. Geipel. there is a doubt as to the lasting powers of high-speed engines, when worked continuously, might it not be advantageous to combine the two systems so that slow-speed engines are used for the light loads, while high-speed engines are used for that part of the plant which is chiefly idle, the first cost of which, owing to its being so large a fraction of the whole, is so important a factor in the cost of supply? For that part of the plant, durability would be less important, of course, owing to the comparatively few hours of use per annum.

Mr. Robinson refers to the difficulty of overcoming the vibration, so far as concerns the one- and two-crank engines, and recommends the three-crank double engine. This, of course, is not a question which greatly concerns the users of high-tension systems, who can generally put their engines in distant places where vibration is of little moment. But is the method of overcoming the vibration advocated not a further argument in favour of the use of slow speed for the light-load engines? I doubt if Mr. Robinson himself would advocate the use of two engines, each having nine cylinders—that is, 18 cylinders in all—for the purposes of driving the light loads.

For tramway work, the author correctly emphasises the necessity of engines considerably larger than are required for the mean power, and in paragraph 23 he refers to the higher mechanical efficiency of double-acting engines at light loads; I agree with the author in this, and believe that for such work the double-acting engine is best—at least, in this respect.

The necessity of using powerful governors and giving them next to nothing to do, mentioned in paragraph 49, cannot be too strenuously dwelt on. The makers of throttle-valve governors are too prone to cut down the weight of the balls. There is no part in an electric light engine which is more important, and to which probably less attention has been paid hitherto, than the governor. I agree with the author as to the desirability of using plenty of fly-wheel power to control the engine in the event of sudden variations in load, though I do not believe that the fly-wheel has so much steadying effect on the governor as he states; in fact, if there is any undue friction in the governor, it is

conceivable that the jerky rotation due to a light fly-wheel would assist the balls in overcoming any extra friction, which might otherwise cause hunting. The great sensitiveness of shaft governors is, in my opinion, partly attributable to a continual variation in the strains on the parts controlled by the balls: probably at certain moments in each revolution there is comparative absence of friction, so that the balls are free to move the eccentric to its proper position. Anyhow, speaking from experience, I have found shaft governors, notwithstanding the heavy work entailed on the balls, to give closer governing than throttle-valve governors. Mr. Geipel.

Mr. J. N. Shoolbred: There are one or two points in this valuable and interesting paper to which I would like to draw attention; having myself had the advantage during some length of time past of seeing these engines at work. In doing so, however, I am afraid that I cannot eulogise the Willans engine in the same wide terms as Mr. Wright has just done. Mr. Shoolbred.

One point is raised in the paper, and it is one that has been recently discussed before the Institution of Mechanical Engineers, viz., the question of expansion *versus* throttling valves.

The large amount of throttling of the steam required with the Willans engine has always seemed to me to be a certain blemish in the use of that engine; for it practically means that, in order to use the Willans engine economically, it must be kept pretty fully loaded. It does, besides, seem rather anomalous that steam should first be demanded at a very high pressure, in order then to throttle down that steam, and make use of it at a loss in its pressure of from 25 to 75 per cent. The remedy for this throttling of the steam pressure with light loads lies in keeping the engine as fully loaded as possible. Several expedients may be made use of to effect this object,—one being a suitable arrangement in the relative sizes of the engines used in central stations; the various sizes selected being such, that the effect of the successive addition of engines with the increasing load, or of withdrawals with the diminishing load, is to ensure that the engines at work shall always be loaded somewhere between 75 per cent. and the full of their power.

Mr. Shoolbred.

Another valuable adjunct in effecting this "full-loading" of the steam engine is found in the aid given by the storage battery, which can be thrown in to the assistance of the engine, or not, as required. References have been made of late, in the newspapers, to the assistance thus afforded by storage batteries at the central station at Pontypool; the special interest being due to the smallness of the installation, and to the economical results, in working, which seem to have been arrived at. In estimating, however, the financial advantages to be derived from these storage batteries, the original value, as well as the amount of their maintenance, should not be lost sight of. But this is hardly a point for discussion in connection with the present paper.

There are, however, some points, such as absence of vibration, of silence, and of steadiness when running, upon which the Willans engine is more satisfactory in working than is the double-acting vertical engine—at least, with such engines as I have had an opportunity of comparing them. The vertical double-acting engine, even of good recognised make, has proved itself to be inferior to the Willans in the above particular cases—much to the surprise certainly of myself, and also of others who anticipated quite a different result.

The Willans engine is sometimes spoken of as "the engine of "the future;" it appears to me, however, rather to be "the "engine of the present." I cannot but think that, the more we get into larger sizes of engines, the more the double-acting engine will be used—more so even than the Willans—and for this reason: At the present moment. we will say between 50 H.P. and 500 H.P., the Willans engine, being single-acting, has only half the number of steam faces that the double-acting engine of similar power would require, and in the latter engine these would, in some cases, become inconveniently small; not to speak of the various valve gear in the double acting type of engine.

With the double-acting engine, however, in the larger sizes—say from 500 H.P. to 1,000 H.P., and over—the increased number of steam faces would become an advantage, as each of them would be relatively small in size and more handy, when compared with those of the single-acting engine. Hence, possibly, there

would be a more extended use of the double-acting type in these larger sizes. Mr. Shoolbred.

In this view I am supported by several well-known makers of marine types of vertical engines, who, in conversation with myself, have admitted that below 500 H.P. the Willans engine has a superiority, but above it—say up to 1,000 H.P. at least—the vertical double-acting will prove probably the more successful. As to what may be the type of the future for engines above 1,000 H.P., for use in central stations, I cannot pretend to say.

In any case, whatever be the particular value of the Willans engine, we must all admit that it has rendered during the early days of electric lighting invaluable service. It has filled the gap which presented itself to us, when we found ourselves at the limit, so to say, of the earlier used agricultural engine, and when we had before us the larger and more complicated problem of central station lighting; filling a void, and rendering us assistance in a way in which no other engine, then available, could have done.

In conclusion, I may, I think, venture to say, that, whatever the merits or demerits of the Willans engine may be, all of us who are interested in the practical applications of electrical science must acknowledge, that we owe a debt of gratitude to the ingenuity and mechanical skill of the late Peter William Willans, and likewise to his *collaborateurs*, who have so ably seconded him by further developing and improving the single-acting engine, which was the outcome of his scientific researches.

Mr. R. W. ALLEN: As a designer of double-acting engines for many years, I should like to give Messrs. Willans & Robinson credit for setting us the example, not only in designing double-acting engines, but also for the great excellency of manufacture, which is a most important point. I lay great stress on the crank-shaft of an engine, whether single- or double-acting. The crank-shaft is really the basis of an engine. There are very few men who can really turn a crank-shaft true; and unless a crank-pin is true, then there is no doubt trouble will take place. Moreover, it is impossible to obtain these high efficiencies which have been shown to us unless the greatest care is taken in the manufacture of every detail. I Mr. Allen.

Mr. Allen. should like to make one or two remarks about Mr. Morcom's observations. There is no doubt his engine has only been designed in the last couple of years, and that when he designed his engine he did as we have done ourselves: we have had a section of a Willans engine in front of us, and taken great advantage of Messrs. Willans & Robinson's experience of the last 15 years. That has not only helped us as a small community, but it has helped the world at large in designing not only small, but large engines. What Mr. Morcom said with regard to the Navy is true, but of course those are circumstances over which I do not think Messrs. Willans & Robinson can have any control. As illustration, I may say that the other day, in a ship in the Merchant Service which had been lying in port for a few days, water had accumulated in the steam-pipe, and the engines were working at full load; and the night before sailing one of the engineers turned on the wrong cock, and, instead of the water going into the condenser, it came right back into the engine, and the engine was completely doubled up. This would take place just the same in a single-acting as in a double-acting engine. I used to look upon the Willans engine as a philosophical instrument, but it has now been proved to be one of the most scientific engines of the day.

Mr. Dumas. Mr. R. Dumas: Although I have not had anything to do with central electric lighting stations, I have had a little to do with the Willans engine on board some of Her Majesty's ships, and, as far as my experience there is concerned, I will only say that the interchangeable system of manufacture employed by Messrs. Willans & Robinson was found to be of very great advantage. It is only fair, however, to state that my experience was gained on contractors' trials; and though some of these (due to difficulties with main engines and boilers) were spread over a considerable period of time, yet the time was not sufficient to get average results. I believe that since then Messrs. Willans & Robinson have strengthened the pistons.

There are one or two points in the paper to which I should like to call attention. Taking the three points to which special attention is directed in the order in which they are given at the

end of paragraph 11, I will first make a few remarks about the packing rings. With reference to the piston rings, it is rather surprising to find that steam packing has been so successful, since it has been tried, and abandoned, by so many others. The only conclusion one can come to is that some essential point has been overlooked in previous designs. With the piston rings exposed to the varying pressure of steam in the cylinder, it might be expected, in cases where the lubrication is not very efficient, that the wear in the upper part of the cylinder would exceed the wear in the lower part. Perhaps Mr. Robinson, in his reply, will be able to tell us whether he has come across any such cases. That the amount of lubrication has an important bearing on the case is evident from the laws of the transmission of pressure in fluids, and also from paragraph 13, where it is stated that rings of the old design could be started collapsing by increasing the oil supply. With an ample supply of oil, it is probable that no variation in the wear of the different parts of the cylinder would be found. The gland rings would appear to be pressed most against the valve trunk during the up stroke and the latter part of down stroke. Now, with regard to the piston valve packing, Mr. Chandler has already referred to one point to which I wish to call your attention; but I would carry his remarks a little further, and that specially with reference to the packing rings at the lower part of the column of valves. It is evident that the pressure in the interior of the line of valves must be somewhere about a mean between the initial and exhaust pressures. Therefore the pressure at the back of the lower rings must considerably exceed the pressure to which they are exposed outside; consequently the wear at that part is probably somewhat excessive. With regard to the upper part of the line of valves, I have no doubt that Messrs. Willans would be quite safe in guaranteeing that the tool marks would not wear out, say, in five years after the date on which the engine was started to work, for the simple reason, as Mr. Chandler has said, that the rings must start collapsing. But I daresay Mr. Robinson will be able to tell us whether he has found the wear at the bottom to exceed the wear at the top. I suppose the reason why no attempt has been made to subdivide Mr. Dunns.

Mr. Dumas. the hollow valve is that its weight would be increased thereby, and this would increase the difficulties mentioned in the paper in the case of throttling engines working on light loads.

With regard to the internal drainage, the coning of the pistons is at once a simple and efficient way of overcoming the difficulty as far as the cylinders are concerned, and it is a most happy coincidence that the strength of the pistons is increased at the same time. One has only to suppose the coning of the pistons to be of the opposite sense to that adopted in order to appreciate the superiority of the latter. In the one case the piston would act most efficiently in doing the very thing to be avoided, namely, in spreading a thin film of moisture on the cylinder walls; while with the piston coned down at the centre the moisture disappears down the valve trunk.

With regard to the Cornish cycle, I am of opinion that this is a most important and beneficial feature in the engine. I was rather surprised to hear Mr. Tremlett Carter question its advantages; probably, however, he has, after perusing the paper at leisure, altered his opinion.

With regard to the air cushion arrangement, I understand that this is so designed as to keep the lines of parts (of course excepting the line of valves) in constant thrust, even when there is practically no pressure in the steam chest. This being so, it is clear that whatever effective pressure there is in the cylinder is added to the pressure due to the air cushion, and that the pressure on the crank-pin must be very severe during some parts of the down stroke. Probably this has some connection with the crank-shaft breakages referred to in the paper. Since there is so little clearance, and, consequently, compression, in a Willans engine, the valve trunk must for some parts of the up stroke be in tension. I should like to know if there has been any trouble with breakages through the bottom row of ports. Doubtless the air cushion arrangement is very efficient, and it is more easily adjusted than the compression in a steam cylinder. In a double-acting engine the compression curve is often very deceptive, especially when the diagrams from the two ends of the cylinder are separated. Now that variable expan-

sion has in some cases been adopted, and that the full pressure is consequently available on the high-pressure piston for accelerating the parts, there seems to be no reason why an effort should not be made to get rid of the air cushion. This could be done by arranging for the valve itself to cut off somewhat earlier than is at present done, say about 0·6 of the stroke, the release to take place at 0·9, and compression about 0·83. Naturally, the clearance volume, and, consequently, surface, would have to be increased. If it is objected that this would prejudicially affect the economy of the engine, let me ask what has been the effect of letting the steam to the back of the piston rings, and thus considerably to increase the clearance surface. I am of opinion that the mechanical efficiency of the engine would be sensibly increased if the cushioning cylinder could be dispensed with. I am aware that this would not be possible if a late cut-off were required, as sufficient compression could not be obtained in the steam cylinder. The remarks in the appendix on the subject of jackets are very interesting, and it would add considerably to the value of the paper if the percentage of steam condensed in the jackets in the case referred to could be given; and Messrs. Willans & Robinson would earn the gratitude of engineers if a series of trials were made on the same engine over a wide range of revolutions to show the effect of jacketing on steam consumption when exerting the same torque. Mr. Dunlop.

It appears to me that the answer to the question, "Will "jacketing pay?" does not in every case depend altogether on whether the steam consumption is lessened thereby, since there is a small gain due to the hot water from the jackets being available for heating the feed water, and, also, a smaller amount of condensing water is required. With high-speed engines the saving referred to would not be important, as only a small percentage of the steam used would be condensed in the jackets; but with well-jacketed slow-running engines where condensing water is scarce, and with marine engines in hot climates, where the condensing water is warm, the saving would be quite appreciable. In cases where the hot-well discharge is not used for feeding the boilers the amount of feed water to be paid for would be reduced by the amount condensed in the jackets.

Mr. Head. Mr. JEREMIAH HEAD: In the course of this exceedingly interesting discussion the question raised by Mr. Malcolm as to the effective lubrication of the crank-pins and main journals of single-acting engines has revived in my mind the memory of some old experiences. Some 20 years ago, I was so smitten with that beautiful piece of mechanism, the Brotherhood engine, that I not only purchased and set to work several small ones, but I also ordered one much larger than any previously made. It had three 20-inch cylinders, and worked with a pressure of 60 lbs. on the square inch. It was put to drive a rolling mill, and ran at 300 revolutions per minute. I regret to say it utterly failed in the end by the continual seizing of big ends of the connecting rods with the crank-pin. A somewhat elaborate arrangement was made for oiling. The oil was not, however, pumped in. It was merely allowed to run down channels in the vertical piston rod and the connecting rod jointed therewith, which produced a considerable hydraulic head at the crank-pin.

In addition to this provision, a large impermeator was placed on the steam pipe close to the engine, with a view to keeping the steam always charged with oil. As the crank-pin was partly bare, and the close chamber in which it worked was always full of oily exhaust steam, its lubrication was supposed to be greatly assisted thereby. Nevertheless, the engine was a failure, because it could not be kept from seizing at the crank-pin. This experience naturally impressed me very strongly with the view that single-acting engines wherein the pressure on the journals and pins is severe and in one direction only are liable to fail, unless some arrangement is made by which the oil can be pumped into those bearings at a pressure at least equal to that upon the working surfaces. I trust this small item of information may be considered to bear on the subject under discussion, and may be of interest to some of the members.

The President. The PRESIDENT: I had the great honour and pleasure of being an intimate friend of the late Mr. Willans, and I was one of the first people who bought one of his engines. It is 19 years since I bought the first Willans engine, and I have been using them

almost continuously during that period of time. I think that this paper, which is on the subject of high-speed engines, has been unfairly criticised in one technical journal. It is suggested that the writer used his opportunity, in discussing high-speed engines, to discuss the Willans engine only. I think that everyone in this room will agree with me that the paper itself is a complete reply to that. It is perfectly certain that a member of the firm of Willans & Robinson, who have made such a large number of high-speed engines, in reading a paper on the subject, would naturally quote from his own experience rather than from that of other firms. We should not welcome his paper if he gave us hearsay evidence. He gives us his own experience, and that is what we value most. I had over 25 years' experience in charge of steam engines, and during the early part of that time I had unpleasant experiences with high-speed double-acting engines. I was out in India, and I had generally to let the brasses together myself, because native labour was not then sufficiently trained to such work; therefore I, of all men, can well appreciate the difficulties in keeping in order high-speed double-acting engines, in eliminating the troubles connected with bearings, and in keeping them free from knocking and vibration. It was my strong feeling on this point which brought me into early association, first with Mr. Brotherhood—whose name has not been sufficiently mentioned in connection with the paper (for I am sure the firm of Willans & Robinson would be the first to acknowledge the great services that Mr. Brotherhood rendered in the case of high-speed engines)—and afterwards with Mr. Willans; as I have told you, I have been using his engines for 19 years. I will not touch on many points of the paper, which will be dealt with better by the writer in his reply. I do not speak as an advocate of the Willans engine *qua* Willans engine, but as a highly successful high-speed engine; as I feel that high speed has been the making of us electrical engineers, and that if it had not been for high-speed engines being introduced, we should not have made such progress in our central station work as we have done. I think that Mr. Morcom and Mr. Chandler will agree with me when I say that the great

The President.

The President. trade that has been made, and that is likely to be made, in high-speed engines is owing very greatly to the Thames Ditton firm. Mr. Raworth, in his very humorous speech at the last meeting, objected to the Willans combination, because, he said, it was such a puzzle to take down and re-erect. I must say I do not understand this. I am in charge of a great many of these engines, and they have always been taken down and put together again by labour much more unskilled than is usually employed in such cases. There are lots of men who are trusted to take down and put together a Willans engine who are not fitters in the usual sense of the word, and would not be trusted to fit together brasses, and work of that kind, in an ordinary double-acting engine. The reason is that in the Willans engine all they have to do is to take the engine down, and put it together just in the same way as they took it down, and, if there are parts worn, replace them by others. The work is perfectly simple, and the men have only to obey orders. It is not fair for Mr. Raworth to talk of the large number of parts in the Willans engine. He compares the type of engine he uses himself, which is an ordinary marine type of engine, with compound or triple cylinders side by side, with a compound or triple Willans engine which has an extra number of cylinders, because Mr. Willans found it a great advantage to arrange his expansion cylinders tandem fashion. And why did he arrange them in that fashion? Because he found he could not get equal turning moment throughout the stroke with the side-by-side arrangement. I was the purchaser of a Willans side-by-side cylinder compound engine, I think in 1879, and I exhibited it at the R.A.S.E. Kilburn Show. That engine was quite as simple as any ordinary side-by-side compound engine, and had as few parts. I had long talks with Mr. Willans on that subject, and perhaps in that sense he partly owes the application of his engine to me, because I pointed out that for electric lighting purposes we must work our engines at very large ranges of load, and that the turning moment must be quite as even throughout the stroke in the small loads as in the large loads. That at once does away with Mr. Raworth's comparison, which otherwise was a fair one to make. Mr. Raworth

told a most interesting story about a tram-car, a motor, and a horse, comparing the complication of the motor with that of the inside of a horse; and I think he hits upon the real reason for which many engineers, including myself, think that Willans and the other engineers who enclose their engines have done the right thing. What matters it if parts are complicated if it is found in practice that they are effective and do not become deranged in working? The Constructor of the horse was very well aware of this. He knew that the horse was really as simple a machine as could be designed for its purpose. But He knew that people would object to seeing the working parts, and He also knew that lubrication and things of that kind are not nice things to fly about in public. He therefore encased it; and I think that is sufficient justification for Messrs. Willans, Chandler, and Morcom to enclose their engines in the way they have done; and therefore we do not mind whether they are complicated or not inside, so long as the steam goes in and the power comes out and they do not require repairs. I will just give you another illustration on this point. I am sure there are mechanical engineers present who will thoroughly agree with me when I say that, from the English engineers' point of view, the practice of some Continental engineers, of putting all the works of their engines outside, is equally a violation of engineering good taste. I think that in enclosing our engine works, as the firm of Willans and these other gentlemen have done, they have done as good service as the makers of Great Northern locomotives have done, as will be seen when we compare their neat locomotive with those of the Chemin de Fer de L'Ouest—those locomotives many of you no doubt have noticed before your eyes as you land at Havre or Dieppe.

The President.

Turning now to the question of economy, the opponents of the high-speed engines are always telling us that, if we want the highest economy combined with the highest durability of the engine, we must adopt Lancashire practice,—we must use large slow-working engines; and we are flooded with leading articles in various technical journals impressing upon us that we electrical engineers ought to go and take lessons at the fountain-head upon all those points. I have challenged all these fountain-head men

The President. to come here and tell us something this evening, and I cannot get one of them to come up. Apparently they have nothing to say, or surely they would be here to back up the articles which they have written. But there is no doubt that their outcry has influenced—and I regret it very much—a number of members of our profession; for there are a number of engines still being put in, the putting in of which is really pandering to this outcry, and which I am sure will eventually be a cause of regret to their users, when they compare their results with the results that we who use higher speed engines are even now obtaining in existing central stations. I think all the speakers this evening are in agreement upon this point, and I regret only that we have not had some of the slow-speed men here. None of the doubts which have been raised so frequently as to the want of durability and the cost of repair of high-speed engines have been found realised in practice. We have now had eight or nine years of central station practice, and we have found that the cost of the high-speed engines is as low as, and in some cases lower than, that of the slow-speed engines. We are getting rather tired of hearing this, and I should like to hear something fresh; I should like to hear really on what grounds these assertions have been made. I regretted to hear a gentleman this evening talking about sweeping up the interior of an engine in a dust-pan. I do not think that is quite a nice way of putting it. There is no doubt that in the early days there were such accidents, and there is no doubt of the extremely rough way in which they treat engines in the Royal Navy. If they inject steam pipes full of water suddenly into the cylinders of the engine, it is far better that something should give way in it, as it may prevent a nasty accident elsewhere. There is no doubt that these accidents do not now occur, and have not occurred for some time; and that any engineer who allowed them to occur in his station would not confess it, because he would know that his experience was so abnormal that he would not like to be thought of as the careless man that he would prove himself to be by permitting such accidents. To give you some idea of how unfairly criticisms are raised, I notice in an article in a technical paper which came

The President.

out quite recently, that the engines now under discussion were abused on the ground that Mr. G. M. Clark, in those highly interesting figures, had stated that the consumption was 46 lbs. in practice, whereas it was only 32 lbs. in test. Well, now, Mr. Clark absolutely proved identically the reverse of this. He proved that the difference between the 32 lbs. and the 46 lbs. was all to be explained by causes outside the engine, with the exception of that quantity due to the change of load-factor of the engine, which could be foreseen and calculated for; and he actually reproduced the very identical figures that I was able to produce in my Civil Engineers paper six years ago, which really did show that our figures at Kensington, after several years' working, were identically the same as we got from the engines when tried in test. And Mr. Clark, who is a most careful observer, reproduced the most interesting table before the Mechanical Engineers which has been copied into Mr. Robinson's paper, and yet a leader writer takes this as proof that these engines fall off in efficiency from 32 to 46 lbs. I only do say this—that I think we have every reason to be extremely proud of what the Thames Ditton firm, as well as Mr. Belliss and Mr. Chandler, and other makers of these high-speed engines, have done for us. They have enabled us to show figures in steam into electrical units which are better than any figures I have ever come across. I sent out a specification the other day for machinery to be competed for by firms other than my own, and I think the figures in all cases were very close—nearly as good as those figures that are shown on the table given by Messrs. Belliss—and not only did they undertake to give the machinery which would work up to that high standard of economy, but they also were prepared to guarantee it for a year and not take the whole of their money until they had proved their figures. I have had a Continental engineer of high repute over here for some time past, and he would hardly credit me when I showed him my specification for steam dynamos: He said: "You will never get tenders in for that "specification; and if you tell me that your English engineers "can guarantee such figures as you ask for, I shall be converted

The President. "to English practice at once." He will be back in London in a few days, and then I shall be able to show him the tenders. I think that is the best proof that high-speed work, for which we are so greatly indebted to Messrs. Willans & Robinson, has been a very great movement in advance for the benefit of engineering generally and for electrical engineers in particular; and for that reason I am extremely pleased that our last paper of the session has been on such an extremely important and interesting subject. I will now call upon Mr. Robinson to give his reply.

Captain Sankey. Captain H. R. SANKEY [*communicated*]: I would like to refer to Mr. Booth's contention that ordinary compound receiver engines could be arranged so as not to have any overlap of pressures (or temperatures). It is to be presumed that Mr. Booth meant that the overlap was small, and thus practicably negligible; because it is evident that, theoretically, in such engines overlap must occur, depending in amount on the admission and exhaust pressures, on the ratio between the cylinder volumes, on the cut-off in the cylinder, and on the setting of the cranks. Practically, however, throttling of the steam through the ports, leakage and radiation caused a drop of pressure between the exhaust of the high-pressure cylinder and the admission of the low-pressure cylinder, and those losses would reduce the overlap, and even expunge it altogether in some cases.

This reduction of the overlap was distinctly noticeable in Figs. 3 and 4, as would be seen by combining the two diagrams. Unfortunately, Mr. Booth had only given one diagram in each of the other cases, so that no definite opinion could be given; but in all of them indications of overlap were clear. This was so especially in Fig. 5. There the exhaust line *rose* from 16 lbs. pressure (251·5°) to 28 lbs. (271°). In a Cornish cycle engine the exhaust would have been constantly at 28 lbs., and the range of temperature in the cylinder would have been reduced by $271° - 251·5° = 19·5°$ Fahr., assuming the same admission pressure—a very considerable gain for the Cornish cycle.

Mr. Robinson has referred to certain breakages of crankshafts in Willans engines, and said the reason was the very simple one

that they were not strong enough. Originally these shafts had been proportioned from those in general use in double-acting engines, but allowing a margin. Thus, Professor Unwin gives a rule in his book on "Machine Design," from which it appears that for a 200-I.H.P. engine, running at 350 revolutions, the shaft ought to be $3\frac{3}{8}$ inches diameter, and 4 inches was the diameter adopted. When the first shaft broke at Manchester Square, Mr. Willans asked me to go into the matter; and I found, by determining the variations of the stresses, and especially taking account of the bending and shearing stresses, that there were two points where not only the stress, but more particularly the variation of stress, was a maximum; and these are the two weakest points in the crank-shaft. These two points are marked a and a_1 in Fig. A, and it Captain Sankey.

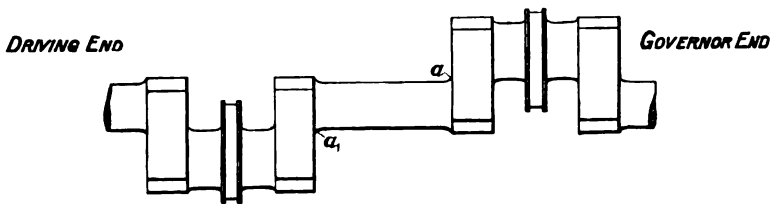


FIG. A.

is worth remarking as a confirmation that, with three exceptions, due to undoubted flaws elsewhere, the crank-shafts that have broken have done so either at a or at a_1, and about equally at each place, as required by the calculation.

It is probably unnecessary for me to state that for a long time past the shafts of these engines have been made of ample strength.

Mr. Tremlett Carter contended that, notwithstanding the separation of temperatures due to the Cornish cycle, the cylinder walls were exposed to the full range of temperature, and that thus no advantage was gained in respect of reduced initial condensation; and he illustrated his point by considering a certain portion of the cylinder wall. But the particular portion he had considered had little effect in producing condensation, the steam coming in contact with it somewhat late in the stroke. The metallic surfaces in the cylinder which cause the greatest condensation are the top of the cylinder, the top of the piston, and a small annulus of the cylinder wall—in other words, the initial surfaces;

Captain Sankey.

and these were not exposed to the full range of temperature. The portion of the cylinder wall covered by the piston was also only subject to this reduced range. On the whole, therefore, the Cornish cycle materially reduced the range of temperature. This matter is illustrated in Fig. B, the cross-shaded part of the cylinder

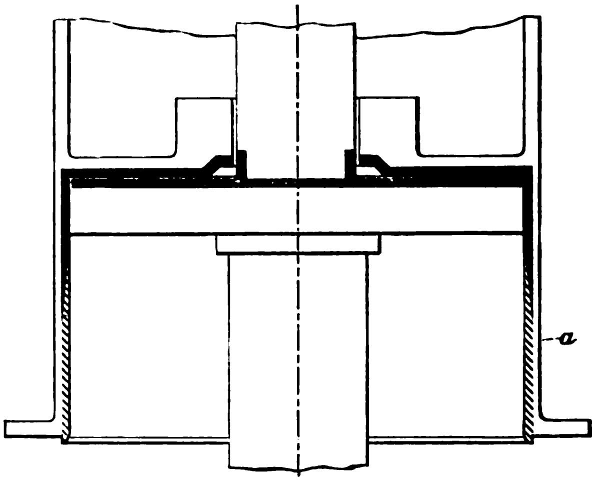


Fig. B.

being subjected to the full range of temperature, and the point *a* is about the position of the portion of the cylinder wall chosen by Mr. Tremlett Carter. The black portion is exposed only to the reduced range. It is clear that the gain is considerable.

Dr. Preller.

Dr. Du Riche Preller [*communicated*]: In the course of the discussion on electric traction at the beginning of this session, Mr. Robinson attacked me somewhat vigorously because I had stated that, for traction purposes, there were certain cases in which direct driving by small high-speed units was not economical—a contention the accuracy of which in a specific case I proved by actual figures. I willingly admit that the steam engines in question were double-acting engines, and in this, as well as in some other respects, inferior to the Willans engine. But, still, Mr. Robinson's attack was on the general question of direct driving, and he laid it down that, if that question needed discussion at all, it should have been discussed three years ago. I pointed out at the time that the question had not only

been discussed during the last three years, but would probably be discussed for some time to come; and experience has shown the correctness of this view, for Mr. Robinson and Captain Sankey themselves have this winter alone contributed no less than three very admirable and valuable papers in just defence and vindication of their speciality. Assuredly they would not have done so if the subject were beyond discussion. Dr. Preller.

There can be no doubt whatever as to the superior advantages of direct driving by high-speed engines, and more especially by Willans engines; for the Willans engine is a monument of mechanical engineering skill, and, like everything that I have had the privilege of seeing at Thames Ditton, rests on truly scientific principles. But, having regard more particularly to electric traction, it may be well to emphasise the fact that in smaller installations, such as 300 or 400 H.P., for lines of, say, six miles, the ordinary commercial dynamo of 100 to 200 kilowatts output is designed for 450 revolutions at about 550 volts. If, therefore, such dynamos are to be direct driven, the Willans engine must make the same number of revolutions; and the question is, Has Mr. Robinson already reached that point? Because, if not—viz., if the engine cannot be made to run above, say, 300 or 350 revolutions—the dynamo will have to adapt itself to the engine; in other terms, it must be made larger and heavier. The case is, of course, different in larger units.

In his paper, Mr. Robinson refers to the Heilmann electrical locomotive, which I may justly claim to have first made known in this country (*vide Engineering*, June 2, 1893, *et seq.*). The generating compound horizontal steam engine of the first locomotive of this type was, as Mr. Robinson rightly remarks, practically a perfectly balanced engine; but its stroke was too short, and it also suffered from other mechanical defects, which, however, were not the fault of the designer, Mr. Charles Brown, of Bâle (the father of the well-known electrician), but of the makers. In my view, the best type of generating engine for locomotives of this kind would undoubtedly be the steam turbine of the Parsons and, preferably, the Laval type; but, as these machines are not as yet made for such high powers as 1,500 H.P.,

Dr. Preller. I believe that M. Mazen, the able locomotive engineer of the Western of France Railway Company, who is now acting with the Heilmann Syndicate, judged wisely in adopting for the two new and improved locomotives now being built a special type of Willans engine, as an engine having the least possible vibration. As regards this last-named merit, I take this opportunity of saying that when, not long ago, I visited the central station at Norwich, which has six 120-H.P. Willans engines, located in a *detached* and spacious building, the absence of vibration, both in and near the building, was so striking that it was practically impossible to say whether the steam engines were running or standing. Mr. Robinson and Captain Sankey may well be congratulated on this very conspicuous merit of the Willans engine, as they may be on the valuable and truly scientific papers they have so recently brought before this and other Institutions. The consumption of fuel at the Norwich central station works out very favourably, viz., 7 lbs. per unit generated in winter, when the power is fully utilised.

Mr. Hudson. Mr. JOHN G. HUDSON [*communicated*]: Mr. Raworth having in this discussion quoted a remark of mine which might, as it stands, convey the erroneous impression that I consider the mill type of engine unsuited to fulfil the conditions of electric lighting, I wish to state that my remark applied only to the point which was the subject of conversation at the moment—viz., the requirement, real or fancied, that the engine should be capable of running quietly and governing perfectly under full steam pressure, but without any load. I said truly that no mill engine in Lancashire could meet this requirement; and the reason is simple: these engines are never required to run under these conditions, the friction of the engine and transmission gearing alone representing from one-fifth to one-third of the gross load, which is amply sufficient to steady even a condensing engine. Experience has, however, shown that, the requirement being known, it can without much difficulty be effectually met by modifications in detail, which in no way affect the general design of the engine, and are so slight as to be noticeable only by an expert. So far from my thinking the mill type of engine

unsuited to electric lighting, I am distinctly of opinion that it must, on its merits, come to be generally adopted in cases where the gross power installed justifies or requires the use of comparatively large units, and at the same time call for greater economy, efficiency, and durability than are obtainable from small engines of the high-speed type. This opinion has already been largely confirmed by the constantly increasing number of engines of this type which are being adopted or proposed, the majority being of the direct-driving type, with fly-wheel armatures. If further proof is needed of the tendency referred to, it is furnished by the reading of the present admirable paper. Mr. Hudson.

It would be convenient to have a definition of what constitutes a "mill engine," and I would suggest that the essential and only really distinctive feature is the use of some form of trip valve gear controlled by the governor, this feature being so nearly universal in modern mill-driving that there are hardly any exceptions to prove the rule. The reason for this state of things is no doubt the fact that trip gear provides a ready and well-understood method of obtaining such a measure of economy and regularity of speed as have, so far, been unattainable by any positive form of valve gear. Trip gearing has, unfortunately, one disadvantage when applied to engines of small size—viz., that it limits the speed of the engine to that at which the gear can be run, say 100 to 120 revolutions per minute; and though these speeds can no doubt be, and have been, largely exceeded by gear of special design, the limitation will often prevent the application to small engines. Fortunately, there are other, if less perfect, forms of valve gear specially applicable to such engines; whilst in the case of engines of large power, to which such other gears are not suitable, the limitation does not apply, the gear being capable of as high a speed as is desirable for the main parts of the engine, having due regard to safety, durability, and ease of maintenance.

As regards the special subject of this paper—the Willans engine—I have the greatest admiration for the engineering ability which has been brought to bear upon it, and raised it to its present state of efficiency, as well as for the classical papers describing it which were read by the late Mr. Willans before the Institution of Civil

Mr. Hudson. Engineers, and which constitute a mine of valuable engineering information. If I may, however, without offence express my thoughts on the subject, I should wish to point out that the gradual improvement of this engine, extending over many years, is a striking example of the way in which the inherent defects of a design originally far from satisfactory may be overcome, not by any radical change in the original design, but by a lavish expenditure of consummate engineering skill, time, and money, on each and every detail found to give trouble, and by certain additions, such, for example, as the air-buffer cylinder and the steam separator. The undoubted success of the Willans engine must also be ascribed to the admirable system of manufacture and testing; but it must be remembered that much can be done in this way, and in the way of experimenting, with engines individually of small size and cost, which would be wholly impracticable in the case of larger engines, which must, as a rule, be made singly, and generally require to be specially designed to suit the particular conditions under which they have to be used. For the latter reasons, amongst others, I do not think builders of mill engines need feel any overwhelming anxiety on account of the threatened appearance of this new competitor in their particular field; and most mill owners requiring, say, 600 I.H.P. will prefer to obtain this power from two or three cylinders of well-tried and approved design, to adopting an engine of the Willans or other high-speed type, with, say, six cylinders for driving the mill, and six more for driving the air pump, as was done in a recent instance.

Mr. Robinson. Mr. Mark Robinson, in reply, said: Dealing with the remarks of the various speakers in order, I note that Mr. Raworth suggested that I might have treated of other single-acting engines than the Willans; but the answer is in the title of the paper—"On the *Recent* Development of the Single-Acting High-Speed "Engine for Central-Station Work." I have confined myself to the experience and the improvements of the last three or four years; and if I have dealt with no other single-acting engine, it is because none, so far as I know, has yet taken a prominent part in central-station work in this country. I tried to make the paper as general as I could, but it would be affectation to ignore

the fact that for central-station work in England the single- Mr. Robinson acting engine is the Willans engine; in that fact lies the chief reason why I am able to speak to you on the subject. No one can admire the Brotherhood engine more than I do, and it was that engine which directed my attention to the single-acting principle, and led to my connection with Mr. Willans. But Mr. Willans's engines always differed from the Brotherhood type in this—that, whereas the latter has only the connecting-rods in constant thrust, his engines had *all* brasses so arranged, including those of the main bearings. The difference is vital. I think Mr. Raworth unconsciously does an injustice to the early type of Willans engine used in launches. In its compound form it was, for those days, an efficient and economical small engine; and, with the addition of air-cushion cylinders, it was not only largely used for electric lighting, but it was certainly at the time the most economical high-speed engine so applied. As regards the horse-power at work, which Mr. Raworth referred to, it is no doubt difficult to collect such figures, but the list relied upon was likely to be as fair to one type as another. It omitted many unfinished stations, including the large one at Bankside, and this may have cut down the Brush figures, as it did also the figures for single-acting engines, which will be largely employed in the same station. The figures given for single-acting engines also excluded many thousands of horse-power, which did not, in my judgment, come under the head of engines for central-station work for public lighting. Mr. Raworth complained that the Willans engine is such a Chinese puzzle; you have to take it to pieces to get at the inside. Well, I have heard so much of this, that for years I have been on the look-out for the properly opened-up engine that shall have its pistons outside the cylinders, and its valves not hidden away in valve chests; but it has not appeared yet. Even in Mr. Raworth's engines you have still to undo nuts before you can get at things. I admit this is so in the Willans engine, but it shares the defect with its rivals, after all; and there is some evidence in the paper that when you want to get at the inside of a Willans engine you need not be very long

Mr. Robinson.

about it. I am afraid I have not been clear as to the relation between high speed—that is, for continuous running—and "constant thrust." Those who make the Willans engine believe that the primary cause of its successful running at high speeds is the "constant thrust" on the brasses; and it is in accordance with common knowledge that the double-acting engine, which does not work in constant thrust, has not hitherto been able to work at equally high speed with equal success. We are now told that, by a special system of forced lubrication, means have been found for putting double-acting engines on the same footing as single-acting engines, in regard to absence of wear at high speeds. This may, of course, be the case, but I think we may fairly ask for a good deal more time than has yet elapsed, first, before we regard so novel a claim as established, and, secondly, before we admit that the special means by which success is said to have been attained are in themselves free from objection. Mr. Raworth differs from me so widely that he regards constant thrust, not as a necessary condition of success in making long runs at high speed, but as an absolute disadvantage; and he referred to some lightly weighted bearings of Mordey alternators, necessarily in constant thrust, in which there had been trouble. If there is really trouble with these very light pressures per square inch, the notoriously successful running of the Willans engines, in constant thrust, and under far heavier pressures, may well be set against it; but I also venture to suggest to Mr. Raworth to look for other possible causes in this special case. I have seen the same difficulty with bearings apparently quite lightly loaded, where the shaft formed part of a magnetic circuit; and the conclusion come to was that, although the pressure between the shaft and the bearing might appear to be light, yet, through magnetic pull, it might really be very heavy, and the cause of serious trouble. But Mr. Raworth not only values constant thrust thus lightly: he sees the true cause of the success of the Willans engine in the short stroke; at least, he says that its high speed is "due to" the short stroke. I have a difficulty in following this; but in any high-speed engine shortness of stroke is desirable:

long stroke would add gratuitous difficulties. On the other hand, as Mr. Raworth points out, in an ordinary double-acting engine short stroke introduces other difficulties, affecting economy; so the moral appears to be that, if you want an economical high-speed engine, you should use a single-acting one—a conclusion which, whether or not it is really Mr. Raworth's, certainly commends itself to me. Mr. Raworth's interpretation of my reason for preferring double action for *extreme* speeds in large engines—speeds at which you sacrifice everything to speed—is interesting and acute, but it goes beyond my real meaning, which was merely that constant thrust is not easy to maintain, and that at extreme speeds it becomes altogether too difficult. Mr. Raworth is mistaken in supposing that exceptionally high brake efficiency is claimed for the Willans engine—that is, in comparison with other vertical engines of good design. The claim is only that all such engines are pretty much alike, including the Willans; the latter atoning for a doubtless larger share of ring friction by superiority in other directions. It must not be forgotten that, in one sense, the worse the engine the better its brake efficiency ought to be. A common single-cylinder single-crank engine, however wasteful of steam, should clearly show higher brake efficiency than a compound engine of the same power, with two cylinders and two cranks: brake efficiency is but one element out of many, which make for or against economy. On the figures quoted by Mr. Raworth I do not feel able to speak, though it seems to me rash to claim 93 per cent. for the engine on no better ground than that the efficiency of the Mordey dynamo could not be more than 90. On one point I would like to correct Mr. Raworth. With too much kindness he gave credit to myself and my colleagues for the recent improvements in the Willans engine, whereas those improvements were all either planned by Mr. Willans before his death, or were based upon the experiments which he instituted and carried out. The only exception is the automatic expansion gear now used. Mr. Robinson.

Mr. Chandler was quite right in saying that the old piston ring he showed on the blackboard was identical in principle with

Mr. Robinson.

that which works so well in the Willans engine. Its action is just the same, *provided the engine is single-acting.* In a double-acting engine it would have no merit,—which is another of the innumerable indirect testimonies to the advantage of a single-acting engine. The spring follower used in the Willans engine, which is wanting in the old example shown, is a useful safeguard against accidents. With regard to pressure behind the valve rings, no doubt Mr. Chandler is correct; there is some sort of average pressure there, intermediate between the steam and the exhaust pressures. Perhaps one ought to know more about it; but, since the valve rings never give any trouble, one accepts the fact that the pressure, whatever it is, is enough for the upper rings, and not too much for the lower rings, and one is content to leave the subject alone. I would certainly have referred to the Chandler engine in this paper had I been in possession of facts about it. Not very much has been published about its economical efficiency, but I am glad of the opportunity of saying that all I have heard of the Chandler engine has been very much in its favour.

Mr. Booth's objection to the Cornish cycle—or, rather, to the claim that it secures a better division of temperatures in all the cylinders—has been dealt with by Captain Sankey. No doubt, with two cranks at 90°, and early cut-off in the low-pressure cylinder, you can avoid having the high-pressure cylinder open at any time to the low-pressure, and the consequent overlap. But such an engine is not necessarily the most convenient for an electric light station. If there are two cranks, they should be opposite to each other, for vibration reasons; and that, in a double-acting engine, introduces the Woolf cycle at once, in which overlap is undoubted. With three cranks—the best arrangement of all—you cannot avoid two consecutive cylinders being open at the same time to the same receiver, which, again, means overlap of temperatures. I adhere to the view that the use of the Cornish cycle in all the cylinders, which is impossible in a double-acting engine, gives a considerable advantage to the Willans engine.

Mr. Tremlett Carter's objection to the efficacy of the Cornish

Mr. Robinson.

cycle, to which Captain Sankey has also referred, gives one more incidental proof of the advantages of the single-acting high-speed engine. Single action allows the Cornish cycle to be used; high speed brings in short stroke; and Mr. Tremlett Carter's objection, which as against a long-stroke engine has a certain validity, is found to be practically ruled out because the stroke is short.

Mr. Morcom has treated the discussion very much as a duel between single action and double action; and that, I think, is fair enough, because the paper is in praise of the principle of single action. Mr. Morcom says that his firm have made high-speed engines, as I gather, of large power, for 30 years. I presume some of those would be marine engines.

Mr. Morcom: The large majority are marine engines.

Mr. Robinson: In spite of this long record of success, I am not surprised that Mr. Morcom speaks of continuous running in station work as "our difficulty," or that he turned first to the single-acting engine; and in spite of what he saw in the Navy, I am not sure but he will find some day that first thoughts were the wisest. As to the troubles with the Willans engine in the Navy, I could say a great deal, but this is hardly the time or place for a long examination of the subject. The engines were very roughly used on board ship, sometimes through neglect of lubrication, but more often through neglect of the most ordinary precautions in regard to draining the steam and exhaust pipes. In the old days which we are talking about, others who had made auxiliary engines, more experienced than we in the ways of the men of the sea, knew what to expect, and left plenty of places for the water to go through (steam did not matter); and so auxiliary engines made very fair steam traps, so much so that some engineers came to rely upon them as such. I fear I once gave offence at the Admiralty by saying that our engines were steam engines, not hydraulic engines.

No doubt there were other troubles as well. For instance, Mr. Dumas's wish to get rid of the air cushioning was long since anticipated by the engineer of a certain ironclad which I visited. Even before we reached the dynamo room we heard a

Mr. Robinson

tremendous noise, as of steam hammers, and one of the engines was found to be pounding away furiously—near the breaking-up point, I should say. I asked if they did not think the engine a little noisy. No, they did not; they are not always exacting on board ship. However, I went to look for the cause, and found it. Air relief cocks are fixed on the air cushion cylinders in order to ease the starting, and, with the best intentions, they had taken them to be drain cocks, and had coupled them to the dynamo room drains, and then *left them open.* Of course in time the Navy engineers learnt to deal with all these difficulties, but that helped our successors more than ourselves. It is scarcely the case that the single-acting engines had the same attention as the others in the Navy, for they preceded the others, and smoothed the way for them. It is news to me, however, that the engines "failed right and left," or that they have been taken out upon any large scale, except in connection with such changes as always go on in war-ships in the course of years, such as re-boilering with higher pressures, and rearrangements generally. We often receive very satisfactory accounts from naval engineers who have the engines in use. Before leaving this subject, it may be of interest to mention that in these, after all, not very serious collapses there was rarely a case where the breakage of the piston caused much inconvenience. The pieces simply tumbled down to the bottom of the cylinder, and in some cases stopped there until the next quarterly inspection revealed them. The engines were, of course, tandem engines, and if either a high-pressure piston was broken by water from the steam pipe, or a low-pressure piston by water from the exhaust, the unbroken piston usually continued to run the engine, sometimes without anyone knowing of the accident.

Mr. Morcom says there must be double area of piston and of cooling surface, and double stresses on the pins, in a single-acting engine, when compared with a double-acting one. That, of course, is very true, *if they are to run at the same speed;* though, even if they do run at the same speed, and with equal mechanical success, it is not conclusive against single action, which has so many economical advantages. Ability to run at

Mr. Robinson.

equal speed may, nevertheless, be taken, for the present, as the point which needs to be threshed out. Can you without great disadvantages run a double-acting engine (in continuous running, that is, perhaps for months at a time) at the same high speed as a single-acting engine? I say that experience shows that you cannot. Mr. Morcom says that further experience will show that you can. Time alone can decide. Mr. Morcom, criticising constant thrust, rightly attaches importance to the play of the crank-pin, or of the shaft, in its brasses, as assisting the introduction of oil between them; but, on the other hand, there are Willans engines of something like 100,000 H.P. at work, and they are running almost absolutely free, not only from troubles, but even from ascertainable wear in the brasses. Measurements after years of working have given extraordinary results in regard to absence of wear—results such as no double-acting engines working under similar conditions can show. Merely theoretical objections to constant thrust are therefore out of court.

I can congratulate Mr. Morcom on his St. Pancras figures, which are very good; but I do not think he is correct in saying that they are as good as can be given by single-acting engines—not, I think, by some 2 lbs. per E.H.P. The combined efficiency is remarkable, and, in fact, difficult to accept; but all I would say is that, owing to the uncertainties of measurements, especially in a commercial engine room, the makers of the Willans engine have always hesitated to rely upon extreme figures, upon the strength of only a limited number of trials. The highest of these St. Pancras efficiencies just about corresponds with what has been occasionally obtained at Thames Ditton; but we have made it a practice to say very little about them, for, in fact, we have great difficulty in believing them. However, an efficiency of 92 per cent. seems to me to be not beyond what might be expected from a good engine, very well lubricated. Mr. Morcom's allusion to superheating is not very clear to me. A single-acting engine can run as well without oil in the cylinders as a double-acting engine, and I fail to see why either condensing or super-heating should have special terrors for the Willans engine. In fact, its makers are hard

Mr. Robinson

at work trying to advance the super-heating question, and to make the most of what it may yield. The Willans engine's "general "capability of putting oil into the boilers" fairly puzzles me. It really cannot make oil, and if you do not put any in you will not get any out from it, either into the boilers or elsewhere. If these mysterious words are an echo from the days when the Willans launch engine had trunk pistons, and cylinders open to the crank-chamber, so that the crank-chamber lubricant could find its way into the cylinders, then I can understand; but if this is the idea, the critic is wide of the mark, for this construction has never been used for land engines.

With respect to governing, I will merely remark that, in regard to the number of impulses given per unit of time to the crank-shaft, a double-acting engine with two cranks, 180° apart, has no superiority whatever over a single-acting engine with two cranks, running at the same revolutions, for the simple reason that in the former the impulses are given two at a time, and not separately. Governing, however, is not really in question in this paper, and there is no reason why a Bellis engine or any other fast-running engine should not be well governed if the governor is a good one. With Mr. Morcom's remarks upon vibration I am in general agreement. The three-crank engine he now proposes should give excellent results from this point of view: the only doubt is as to the convenience, in a central station, of driving two cranks from above and one from below. Personally, I believe that in all ordinary cases the freedom from vibration obtainable by the mere use of three cranks at 120° apart (provided the three lines of parts weigh exactly the same) will meet every requirement; and you will then have the further great advantage of the beautifully even drive which three such cranks ensure, in comparison with the ordinary two-crank effect which belongs to the type of engine (though it has three cranks) which Mr. Morcom describes.

Mr. Morcom seemed to reproach me for not having "left his "engine alone." Had I done so, in what purported to be a review of high-speed central-station work, I am sure that, if he had not blamed me, others would have done so, and with

justice. It is his engine which best justifies my last paragraph— my claim that the development and the success of the high-speed engine have fostered improvement all round. His "reply"—which is the name he gives, with a certain forensic license, to his lively attack—is most interesting, and our discussion would have been much the poorer without it. Mr. Robinson.

Mr. Arthur Wright has made, from my point of view, the most eloquent speech ever compressed into so few lines. Happy the lighting station whose engine room has no history. Not long since the engineer of a great London company, with a still larger horse-power of single-acting engines, paid a like tribute. I asked how the engines got on, and he answered that he *never thought about the engines.* It is testimony like this which has carried the single-acting engine so far.

Mr. Halpin spoke of brakes, and had doubts as to certain high efficiencies, which I fully share. Some day I hope to speak with more certain knowledge, for a Froude-Reynolds water brake is just being installed at Thames Ditton, capable of absorbing up to 1,000 H.P., and, so far as can be seen, guarded against every possibility of error. Mr. Halpin's remarks upon the helpfulness of the tremors on the locomotive engine in lubricating the brasses are very interesting and pertinent, and ought never to be lost sight of by engine designers.

Mr. Geipel asked how long a high-speed engine will last: will it last 42 years? Well, it will take me nearly 30 years before I can answer that question; but, so far as the shell of a Willans engine goes, I do not know why it should not last that time, or for ever. As regards the cylinders, one which is mentioned in the paper, if it continues to wear at the rate reported by Mr. Longridge's inspector, I reckon should not begin to want reboring (though this is not a formal guarantee) for about 2,500 years. If an engine breaks up as soon as you start it, you know where you are; and if it gives a great deal of trouble in the first few years of its working, it is not likely to last very long; but if the result of the first few years' running is an almost unexampled freedom from wear, you may reasonably conclude that its life is likely to be a long one. I can assure Mr. Geipel that I have

Mr. Robinson.

never advocated the use of six-crank engines for lighting stations, or for tramways, or for anything else on land, except under very peculiar circumstances, such as existed in one or two cases, where what most people would call no vibration at all has been found to be an annoyance. It is satisfactory to be able to believe that we have found something which will enable us to deal with even such cases as these; but I cannot imagine a case where the use of a three-crank (not two-crank) engine would be objectionable for the day loads merely.

Mr. Shoolbred's remarks upon the absence of automatic expansion in most of the Willans engines made up to the present time, appear to me to be open to a construction which I am sure he never intended, and to be liable to be misread as supporting the old fallacy that if you throttle your steam down from 100 lbs. to 50 lbs. you have in some way lost half of what you have paid for. The engine is a heat engine, and you have still practically the same number of heat units as before. The main change for the worse is that you cannot now employ them so profitably, in an engine which has ceased to be properly proportioned to the load. But there is the same difficulty, more or less, if you reduce the load by over-expanding the steam instead of reducing its pressure: you cannot vary the thing which really wants varying, viz., the size of the engine. Experiment and calculation alike prove that the gain by cutting off earlier, rather than by throttling, is in no case more than a small one, and that at very light loads it actually turns into a loss. This point is fully dealt with in the paper, where the steps taken to combine the advantages of both systems in the new engines are explained. Whether you reduce the power in one way or the other, the result is so bad in *every* engine (in comparison with what either a throttling or a variable-expansion engine may achieve when it works at full load) that the only moral to be drawn is, Do not work with light loads at all, or, if you must do it, do it as little as possible. Mr. Shoolbred opined that the larger the engine became, the better would be the chance for the double-acting principle. We used to think so ourselves, but we have gone on to larger and larger engines, and the result has always been better and better.

Mr. Robinson.

It is simply a question of speed. If you can work a single-acting engine at double, or more than double, the speed at which you can conveniently work a double-acting engine, then there can be no doubt, I think, about the advantage of single action; and even if you do not work at so much as double the speed, the disadvantage of larger cylinders, in the single-acting engine, may well be outweighed by the advantages of more silent running, of freedom from knock, and of reduced wear.

Mr. Allen's remarks, coming from a maker of double-acting high-speed engines, are very kind. As I have said before, if the success of the Willans engine tends to promote the improvement of other engines as well, those who make it are more than content. Many will remember the positive pleasure with which Mr. Willans used to trace the good points of his rivals' plans, and his outspoken praise of them.

Mr. Dumas spoke of steam-packed rings having been tried before, but surely it was not in a single-acting engine; if not, the conditions would be wholly different. He asks if there is more wear at the top of the cylinder than at the bottom. I do not think I have heard of such a case, and am quite sure it is not general. The absence of wear in the trunks is remarkable, and such as there is is not especially at the lower end. Whatever theory may point to, the valve rings do not in fact collapse. There is a trunk in the lobby which was taken out of an engine which had been running for about five years at the Savoy Hotel—one of three large engines; it was taken down a few weeks ago for periodical examination and overhaul. There was practically nothing to do to it, but permission was given to replace one cylinder and trunk, and one set of piston rings, in order that those which came out might be shown here, as the engine was running within a stone's throw of this building. The wear of the cylinder is exceedingly small; while, as regards the wear in the trunk, you can see that the inside of it is in beautiful condition. The actual wear in each piece is stated on the card placed upon it. (I will add as an appendix, B, a copy of a report upon another single-acting engine which was examined after five years' wear, containing exact measurements of the wear

Mr. Robinson.

in every part. It will be seen that the wear was very slight indeed after a run computed at 535,000,000 revolutions.) With regard to the tension on the trunk, upon the up stroke, that is very small—far below the safe working limit of the material. Where trunks have broken it has naturally been at the ports; but all such breakages, I think, have been consequential upon something else, such as the breaking of a piston by water. Mr. Dumas also spoke about the excessive pressure there must be on the crank-pin towards the top of the stroke at starting, when we have the pressure of the air buffer and the steam acting together; but the air buffer is there for the very purpose of starting the line of parts: it is there for the purpose of overcoming the inertia.

Mr. Dumas: I said on some part of the down stroke, and not on the top—I quite agree with you there; on some part of the down stroke, that is, after the inertia has been overcome to a great extent and you have the air pressure combined with the steam pressure.

Mr. Robinson: The air pressure rapidly falls: it follows a very steep curve; by half-stroke there is but little pressure left, and as a fact the air cushion helps to keep the diagram square, and to give you a more even turning moment.

Mr. Head spoke of a large Brotherhood engine in which the crank-pin gave trouble by seizing. But the crank-pin in the Brotherhood engine did not work in a bath of oil; and in any case the point of most immediate interest is that the crank-pins in a large number of single-acting engines, of much larger power, and running at high speeds, do not seize or give trouble of any kind.

Our President referred to the Chemin de Fer de l'Ouest locomotives, and he spoke in terms of moral disapproval of their habit of wearing all their working parts outside. I am sure he will be glad to know that that great company has lately made engines on the English model. I was under one of them the other day, and it was for all the world like an English locomotive: with the fullest sense of propriety it hides away all its working parts underneath.

I have now endeavoured, very inadequately, to deal with the valuable and interesting criticisms the paper has received. I

much lament, however, that we have not had the criticisms of some of the advocates of slow-running engines. Mr. Robinson.

Perhaps I may be allowed to refer to a letter which Dr. Kennedy has been kind enough to send to me. He says:—

14, Old Queen Street, Westminster, S.W.,
23rd May, 1895.

Dear Mr. Robinson—I am very sorry that I was in Germany when your paper was read, and have to go to Scotland for a very important appointment to-night, so that I cannot be present at the discussion. I think there is one word in the paper, on page 454, which might be a little misleading to people who do not know the ways of Thames Ditton trials. You say "*Calculated* addition to steam consumption on "account of low load-factor." Literally this is, of course, correct, but in point of fact the addition is not a mere calculated one, but is based on actual experiments made at Thames Ditton, so that it is a calculated addition whose amount is known experimentally. This makes the matter so much more striking that it might be worth while to point it out, in case the word "calculated" should be at some time misunderstood.

Yours very truly,
ALEX. B. W. KENNEDY.

I do not know whether anyone has been misled, but the table shall be corrected.

I thank you, gentlemen, sincerely for the very kind way in which you have received this paper.

Dr. Du Riche Preller, in his very kind communication, says that the ordinary commercial dynamo, up to 200 kilowatts, is designed to run at about 450 revolutions; and he argues that if this exceeds (as, in fact, it does) the convenient speed of a fast-running engine of corresponding power, the case for direct driving is weakened by the fact that the dynamo has to be expressly designed for the engine, slower and larger. In the abstract this is right, and perhaps it is only to the almost universal acceptance of direct driving in this country, that we owe the comparatively low standard of speed which prevails among English dynamo makers. However, I rarely find that lowness of speed is put forward as a practical objection to direct driving; even where belt driving is intended, there seems to be little desire to increase the speed of the dynamo. As regards the Heilmann locomotives, I believe the claims of the steam turbines were fully considered, and that want of power, as at present made, was only one of the grounds of objection.

Mr. Hudson's communication must enhance our regret at his

Mr. Robinson.

absence from the discussion, for no one could have put the "mill "engine" case with more ability, or with so much authority. In what he says about governing I of course agree; but the recent applications of the mill engine type to electric lighting seem to me to depend more largely than he indicates upon the concurrent development of the system of fly-wheel armatures. Every natural type which, in the process of evolution, tends to be superseded, seeks to modify in its favour the surrounding conditions, and the excellent idea of building the dynamo upon the fly-wheel has done much to aid the survival of the mill engine in electric light work. But it would seem to have been overlooked that high-speed engines may profit by it even more largely than slow-speed engines; for, with the same peripheral speed obtained from a fly-wheel armature of, say, 6 feet diameter, as is now obtained from one of, say, 16 feet, the advantage as regards dynamo cost will be enormously on the side of the high-speed engine. Hitherto the mill engine, driving by belts or ropes, has at least had the solid advantage of a cheaper dynamo, because it could run the latter at any speed, however high. But in accepting direct driving the mill engine advocates have surrendered this advantage, and for the first time have placed themselves in a position in which the much greater cost of a very slow running dynamo will prove a serious handicap. Even at present the gain from higher speed is probably so great as to more than balance the disadvantage of having to provide separate framing, bearings, &c., for the dynamo; thus the saving upon these items, when carried out for the high-speed engine, as it has already been for the low-speed, will put the former into a greater position of advantage than it has ever occupied before.

Mr. Hudson bases the claims of the mill engine (for electric lighting work) upon economy, efficiency, and durability; and it would be strange if, in all these respects, the great engine builders of Lancashire failed to show a very high degree of excellence. But the question is whether the mill engine cannot be equalled, under these heads, or even surpassed, by engines which in other points are more directly suited to the work; and it was the object of my paper to show that this question is already

answered in the affirmative. As for economy, can Lancashire show a lower steam consumption, equally well authenticated, than that of the Willans mill engine at Belfast, referred to in paragraph 27 of the paper? This, it should be remembered, is not an engine whose suitableness for electric lighting is open to question, or which is to be brought into the electric lighting field merely because of its economy. It is an electric lighting engine already, specially designed for that work, and sometimes objected to on that very ground, when proposed for other purposes. If the Belfast engine, without any alteration, were coupled to a dynamo, it would work with the same economy, the same steadiness of drive, and the same absence of trouble and wear, as it shows in driving a mill. As regards efficiency, if that means brake efficiency, I have made no special claims, but the high-speed engine has no need to fear comparison; while, as regards durability, I have already said so much in favour of the constant-thrust type, that I will only add a caution against a too rapid acceptance of the view that, because wonderful proofs can be produced of the durability of the *old* mill engines (of the really slow-speed and low-pressure kind), therefore the same durability is to be expected from the vastly different form which the mill engine has taken in modern times. When I spoke of the durability of high-speed engines, I was asked if I could answer for 42 years. The inference was that the questioner could do so, in the case of mill engines. No doubt he could, *but not with such engines* as constitute the alternative offered to us for the modern single-acting engine. It is no use to tell us that a beam engine, working with 20 lbs. steam, and making 30 revolutions a minute, used the same brasses for 40 years. It does not prove that the same record will be maintained by the modern mill engine, which now seems to be defined, on the very highest authority, as any sort of engine which does not run too fast to use trip-gear. Mr. Robinson.

I agree with Mr. Hudson's admiration for trip-gear, but, in face of the results obtained with the single-acting engine at Belfast, I cannot, of course, accept the view that trip-gear gives economy (or governing) not attainable by other gears. It gives small clearance and sharp cut-off, but both these features are present in the fast-running single-acting engine described.

Mr. Robinson.

Offence is indeed the very last feeling which can be inspired by Mr. Hudson's generous and eloquent tribute to the labours of Mr. Willans in perfecting his engine in the face of many difficulties and drawbacks. But I do not know why Mr. Hudson should treat the outcome of so many years and labours as applicable to small powers only. The Belfast engine already twice referred to is not a very small engine, as regards power, and it need not be very much enlarged to cover all the ground at present open to us in lighting stations; in fact, a number of engines exceeding the 600 I.H.P. named by Mr. Hudson are in course of manufacture, differing from the other in nothing but size. No doubt, as regards results, the Belfast engine stands alone, for no such large engine has been tried since; but if the interchangeable system has a special merit, it is that one may say, *Ex uno disce omnes*, with just a little more confidence than if it were one's fate to work always upon varying lines.

Mr. Hudson writes of a recent case where a Willans engine of six cylinders drives the mill, and another, also with six cylinders, drives the air pump. I know of only three mills driven by Willans engines large enough to be fairly called mill engines. In two of these a simple form of air pump is driven by ropes from the mill shafting; in the third there is no air pump at all, the vacuum being maintained by other means. Originally, in this latter case, there were certain pumps driven by a small double-tandem compound engine (which also served as the barring engine, as it still does), but it never had six cylinders.

APPENDIX B.

Report on Wear on various Parts of Willans Patent Central-Valve Compound Engine No. 1,015, GG Size, 80 I.H.P., after 5 Years' Work.

The engine is used for lighting the Ferry Works, and for supplying power to the various electric motors. The average run is 13 hours per day, and it works six days per week. The total run amounts to about 1,560 days, or 20,280 hours, or about 535,392,000 revolutions.

WEAR.

The following figures, and the diagrams, refer to the parts in each line which

show most wear. The wear in the other similar part is in each case either the same as stated, or less. Mr. Robinson.

Low-Pressure Trunks.—Wear from gland rings:—

Low-pressure steam gland ring has reduced trunk 0·002 inch in diameter.
Air buffer ,, ,, ,, 0·015 ,, ,,

(The inside wear due to the valves could not be measured accurately, but it amounts to about 0·002 inch.)

High-Pressure Trunks.

Outside wear due to gland rings ... ... 0·003 inch.
Inside wear due to valves (approximately) ... ... 0·002 ,,

Cylinders.—For the wear in these see Figs. 1 and 2.

Connecting-Rod Brasses.—Worn in the crown 0·02 inch.

Cross-Head Pin.—See Fig. 3. Battered in at A by pressure of rib in guide piston 0·001 inch. Wear at B due to little end of connecting-rod, 0·002 inch.

Valve Guide Pin.—See Fig. 4. Wear due to little end of eccentric rod, 0·004 inch.

Connecting-Rod.—Small end bush wear, *nil*.

Eccentric Rod.—See Fig. 5. Wear in the bush in the small end is 0·002 inch in diameter, that along the line A B amounts to 0·03 inch, and that in the strap varies from 0·075 inch to 0·15 inch.

Crank-Shaft.—See Fig. 6. The grooves worn at the ends* are $\frac{1}{16}$ inch in depth at the governor end, and $\frac{1}{32}$ inch at the dynamo end. The wear on the journals varies from 0·001 inch to 0·002 inch, while that on the eccentrics is *nil*; that on all four crank-pins is 0·007 inch, all being quite alike, but they may have been as much as 0·003 inch below standard size originally.† All pins and journals are absolutely round.

Main Bearings.—See Figs. 7, 8, 9. The wear in these varies from 0 at the governor end to 0·016 inch at the dynamo end.

REPAIRS AND RENEWALS.

Cylinders.—To be recessed at top and bottom of stroke of piston rings, in accordance with present practice. The gland ring faces to have ridges taken down.

Glands.—The gland boxes to have the ridges taken down, and to be let together. One new high-pressure and two new low-pressure gland rings to be put in.

Air Buffer Glands.—The box faces to have ridges taken off. Rings perfect.

Trunks.—To be recessed at end of ring travel inside and outside.

Connecting-Rods.—Do not touch.

Connecting-Rod Brasses.—Do not touch.

High-Pressure Pistons.—Do not touch.

Low-Pressure Pistons.—To be let together.

High- and Low-Pressure Piston Rings.—Take the ridges off the springs; otherwise quite good.

Valve Rings.—Renew these (two low-pressure and three high-pressure in each line).

It is not absolutely necessary to renew either the gland or valve rings, but, as we hope to run an equal time without further repairs, it is better to start with rings fitting the present diameters.

FERRY WORKS, THAMES DITTON, P. A. LOW.
16*th May*, 1895.

* Due to the packing employed, which is dispensed with in later engines, a centrifugal gland, causing no wear, being substituted.

† The maximum deviation allowed from standard size.

Upon the motion of the PRESIDENT, a vote of thanks was passed by acclamation to the Society of Arts for the use of their rooms for the last three meetings of the Institution.

Mr Edmondson.

Mr. Joseph Edmondson exhibited his Zero-Torque Electricity Meter, of which the following description was given:—

The zero-torque electricity meter, as its name implies, consists of a zero-reading dynamometer, combined with mechanism for measuring and integrating, at short intervals, the torque exerted on the moving coil by the current going into consumption.

The Siemens dynamometer is familiar to electricians as an instrument of the utmost simplicity and accuracy, giving, when coupled as a wattmeter, absolutely proportional readings throughout any range. Its obvious advantages in these respects point to it—in a form somewhat modified, but retaining all its essential features—as a pre-eminently suitable basis for a supply meter, and numerous attempts have been made to adapt it to this purpose. Heretofore, however, the action of all such instruments has depended on complicated arrangements for making and breaking contact. In the zero-torque meter these are entirely avoided. The moving coil of the dynamometer is in a closed circuit which is never broken. It carries a small steel stop which controls the registering without in the slightest degree interfering with the *free* and *frictionless* action of the instrument.

The motion of the moving coil being limited to an arc of less than 1°, permits the use of a durable steel wire suspension instead of the delicate suspension of the ordinary dynamometer. A torque of about 90° is given to the usual spiral spring every two minutes by a reciprocating quadrant, or "mangle-wheel," driven by a vibrating motor consisting of a magnet and its armature. This motor is controlled by a small gravity-driven pendulum, starting automatically and having a sparkless contact (Oulton & Edmondson's patent).

The action of the instrument is illustrated by Figs. 1 and 2, in which A is an arm pivoted at G, carrying a click working into a ratchet-wheel, and driving the registering train. It receives a reciprocating motion from the vibrating motor. Fixed on its

arbor is the arm B carrying the steel pin C. The flat steel stop D, on an arm projecting from the moving coil of the dynamometer, is free to move between the stops E and F. When the current is Mr. Edmondson.

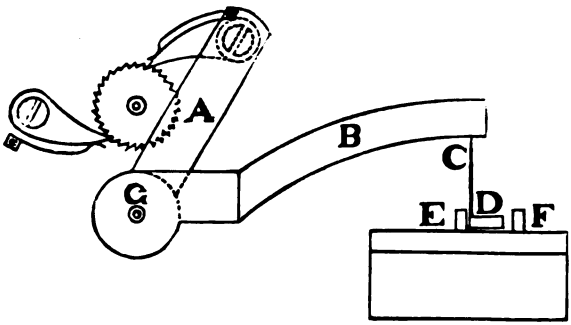


FIG. 1.

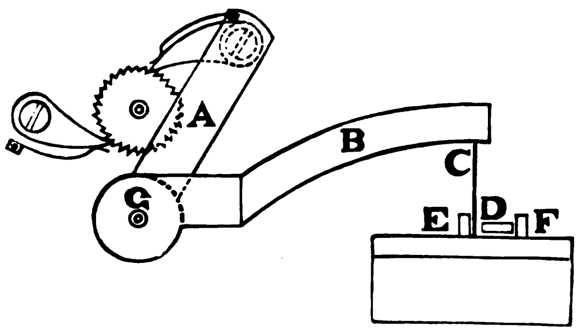


FIG. 2.

nil the stop D remains against the stop E, as in Fig. 1. In this position the pin C comes down upon it at each vibration, and the arm A is thus prevented from vibrating far enough to take a tooth of the ratchet-wheel. The registering is therefore *nil*. But when the force of the current moves the stop D to the right, and it assumes the position shown in Fig. 2, the pin C can descend into the space between the stops D and E, the arm A can vibrate to its full extent, and the registering proceeds. This continues until the reciprocating quadrant, or "mangle-wheel," in its motion to the left, acting through the spiral spring, exerts on the moving coil of the dynamometer a torque equal to that exerted by the current, brings the stop D back to the position shown in Fig. 1, and stops the

Mr. Edmondson.

registering. This action is repeated with each reciprocation of the "mangle-wheel."

As the period during which the registering proceeds is *exactly proportional* to the torque exerted by the current on the moving coil of the dynamometer, the diagram of registration is a straight line.

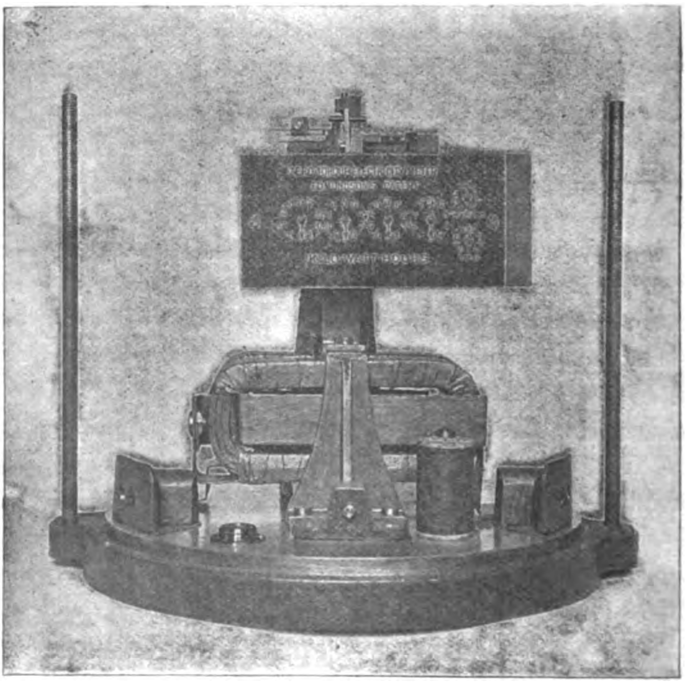
A 25-ampere meter will register from 1-10th ampere upwards.

The total current required for the shunt circuit and the vibrating motor never exceeds, and is generally less than, 2-100ths of an ampere.

The moving coil of the dynamometer, being in the centre of a cylindrical iron case, is shielded from external magnetic influence.

The meter is calibrated to read direct in kilowatt-hours with a constant of 1.

A full view of the meter is shown below.



The PRESIDENT announced that the following candidates had been duly elected :—

Foreign Members:

Clarence L. Cory.
John Kreusé.
Charles Leblanc.
Edward W. Mix.
Albert Petersen.
F. W. Tischendörfer.

Member:

Alfred Dickinson.

Associates:

Harold Stephen Adey.
James T. Cornish.
Charles Jefcoat.
James Edward Lea.
F. Z. Maguire.
Theodore John Ridge.
Arthur E. D. Saul.
Philange Trackson.
Herbert Brandon White.

Students:

Horace Haliburton Bentley.
Charles Flindell.
Martin J. Scears.

The meeting then adjourned.

DAILY INSULATION TESTING OF TELEGRAPH LINES.

By W. H. PREECE, C.B., F.R.S., &c., Past-President.

Mr. Preece. In a telegraph system so extensive as that which is now maintained by the British Post Office, the existence of an accurate method of determining the insulation-resistance of the wires each morning is essential. While the public service demands that the 160,000 miles of open wire, and 28,500 miles of underground and submarine conductors, comprised in the system, shall be ready for the despatch of messages to every portion of the kingdom at any hour, it is necessary for purposes of maintenance and of comparison that all the wires on a particular route may be tested about the same time. In a climate so variable as ours, an hour may produce a considerable difference in the insulation-resistance of a suspended wire, owing to an increase or decrease in the amount of moisture in the air. Hence the operation must be as rapid as possible.

It is inconvenient, and almost impracticable, to test the open lines in short lengths—lengths of 10 miles or less—so as to admit of direct estimation of the insulation-resistance, by disconnecting one end of these short lengths. The tests must be made in sections of an average length of about 50 miles. Even with this distance between the testing offices, and the elimination of all short circuits worked by key duplex (which may be considered to be under a constant test), the number of sections requiring a practically simultaneous test is at present about 1,500.

It is also necessary that this daily test shall secure a fair amount of accuracy. For some time these tests were made by sending a current from a known voltage from one office, and receiving it at the next testing office, on a tangent galvanometer of carefully estimated sensitiveness. To avoid errors due to inequality in the insulation-resistance at different portions of the line, a resistance approximately 10 times the conductor resistance of the wire under test was inserted in the path of the current at each end of the wire, and tables were supplied

showing the mean insulation-resistance corresponding to any given received-current reading. Mr. Preece.

A difference in the conductor-resistance of two wires on the same poles would frequently produce as much difference in the received-current reading on the two wires as would be produced by a small covered-work fault; moreover, the personal equation of the two observers, and inaccuracies in the standards of force and current, introduced elements of doubt, and caused unnecessary verification tests, which occupied valuable time.

The method finally adopted possesses, I believe, sufficient novelty to warrant my introducing it to the notice of this Institution.

It does away with the errors previously introduced by small differences in the "constants" at the testing offices; it reduces the number of tests to one-half; and it facilitates the exact localisation of special faults.

Each morning the minor testing office loops the wires in pairs for a certain number of minutes.

A differentially wound tangent galvanometer, and a carefully estimated E.M.F. of a little over 55½ volts, is employed at the principal testing office. The positive pole of the testing battery is put to "earth," and a current is sent from the negative pole of the battery through one of the galvanometer coils and a resistance of 10,000 ohms, thence through the looped wire, a second resistance of 10,000 ohms, and the other coil of the tangent galvan-

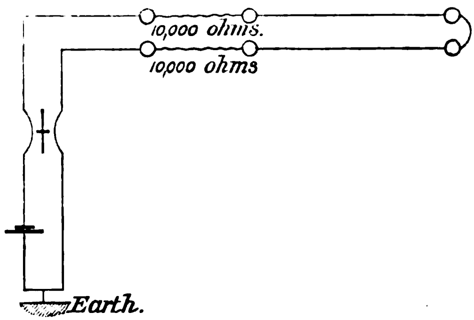


Fig. 1.

ometer to earth (Fig. 1). As the current sent, and that received, are passed through the galvanometer coils so as to exert a

Mr. Preece. deflective force on the needle in opposite directions, it is clear that, in the event of the insulation being infinite, the galvanometer needle will stand at zero; while any difference between the sent and received currents will indicate the exact amount of leakage at the insulators, the mean value of which in insulation-resistance is found by reference to tables supplied for the purpose.

Any considerable difference between the observed leakages on the various loops, on the same route, on the same day, or any small difference existing for two or more days, is carefully localised; and, as the wires are already looped, the usual Wheatstone bridge test for the exact distance of the fault can be promptly applied.

It will be understood that in fine weather the effects of even small covered-work faults are at once seen by comparison with the readings obtained on other similar loops. The deflection observed is wholly due to leakage, and may double the galvanometer reading, under circumstances that, with the original received-current test, would have only produced a difference of 2 per cent. in the observed received-current reading—a fact the importance of which will be recognised when it is stated that a few seconds only can be devoted to this preliminary test of each section.

But comparison between the various loops is not the only means which the testing office has at its disposal for the rapid estimation of the condition of the wires.

A testing office whose bridge may not be at once available can, by changing the position of their testing switch so as to use the galvanometer differentially, instantaneously estimate the difference in tangent divisions between the leakage on the two wires composing the loop (Fig. 2); and the value in tangent

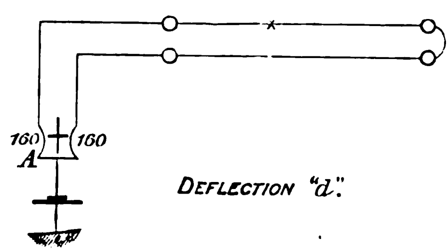


FIG. 2.

divisions of the loss on the wire showing the heavier leakage can also be estimated (Fig. 3). Mr. Preece.

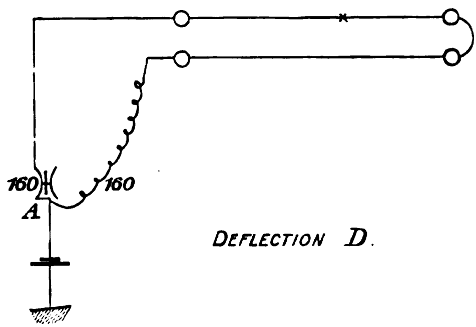


FIG. 3.

If the extra leakage is caused by a definite fault, its distance in ohms can be estimated, with sufficient accuracy for practical purposes, from the readings obtained, and the known mean conductor resistance of the section tested.*

Besides this "maintenance standard," the testing office is supplied with another standard deflection for each section, which requires a little explanation.

Of the various methods of signalling which are employed on the Post Office lines, including key-duplex, quadruplex, multiplex, and even telephonic communication, none require a higher comparative insulation resistance than the Wheatstone automatic high-speed system, whether worked as simplex or as duplex. With this system, condensing compensation for the electromagnetic inertia and self-induction of the line and apparatus is required; and, although the speed with high insulation-resistance is inversely as the K R of the circuit, a careful series of experiments has demonstrated that our lines will not give the full speed value to be obtained with any given K R, and with our present form of apparatus, if the absolute insulation-resistance falls below the total conductor-resistance of the circuit concerned.

As this obviously means that the mileage insulation-resistance must increase as the square of the length of the circuit, the

---

* $\frac{\text{Total loop R} \times \text{D}}{2\,\text{D} - d} - 160 =$ distance of fault from A.

Mr. Preece. required absolute insulation-resistance of two sections tested between the same points, but normally used on circuits of different lengths and resistances, would differ very greatly. The deflection equivalent to this particular insulation is called the "working standard," and indicates the leakage level below which the wire becomes more or less unsuited to our most exacting requirement, namely, automatic high-speed working.

The testing officer has therefore a timely warning as to the rearrangement of sections which is necessary to ensure the required insulation-resistance for the more important wires.

Circuits used for hand signalling only, are workable at a much lower insulation level than that which is necessary for high-speed working, while telephonic loop circuits are capable of being used efficiently when the mileage insulation resistance is very low, if the difference between the insulation-resistance of the two wires is not great.

A few of our 200-mile circuits still contain a considerable proportion of old high-resistance iron wire, and, as the necessary mileage insulation-resistance for high-speed working in these cases exceeds a megohm per mile, it will be seen how important it is to the Post Office to use wires of low resistance.

Indeed, the difference between the recorded deflections corresponding to the minimum maintenance-standard, and those corresponding to the working-standard, at once indicate the particular circuits which require to be reduced in conductor-resistance when renewals are in progress.

This system has been designed and established by Mr. A. Eden, one of the technical officers of the Post Office; and it may be said that the method of test is not only rapid and reliable, but the permanent particulars supplied for each section furnish a complete index to the actual condition and capabilities of the circuit.

ABSTRACTS.

K. MACK—DOUBLE REFRACTION OF ELECTRIC RADIATION.

(*Wiedemann's Annalen, Vol.* 54, *No.* 2, *p.* 342.)

Among the most interesting phenomena in connection with light is that of double refraction, and the author remarks that up to the present time no experiments appear to have been made to investigate the question of the occurrence of similar phenomena in connection with electric rays. The author, combining the information given by Hertz—that these rays go through wooden doors—with the well-known fact that wood has a different structure in the direction parallel to the grain from that which obtains at right angles to it, tried this material with success, using thick plates of fir and other woods. In the method of research he adhered to the following experiment of Hertz:—If two concave mirrors with vertical parallel focal lines face one another, and an electric ray starts from one of them and illuminates the spark gap of the other, when the receiving mirror is turned through an angle of 90° about the ray as axis, the spark is extinguished; if now a wire grating be placed across the path of the ray in such a manner that the wires are inclined at 45° to the horizon and at right angles to the ray, the spark recommences. This phenomenon is analogous to the illumination of the dark field of two crossed Nichol prisms by a tourmalin plate, when the direction of the axis of the latter bisects the angle between the former.

A sufficiently thick plate of fir wood (15 to 20 cm. at least) in which the grain is perpendicular to the thickness acts just in the same manner as the wire grating above mentioned. If the mirrors stand as above (with the focal lines at first vertical, and 2–4 metres apart), the secondary spark is not extinguished if the thick plate is put in the path of the ray and at right angles to it, whether the direction of the grain be vertical or horizontal, although in the former case the diminution in intensity is greater than in the latter.

If now the receiver be turned through 90°, the spark is extinguished in all circumstances, but immediately starts again when the wooden plate is turned through 45°, so that the line of grain bisects the angle between the focal lines of the reflectors. As far as the thickness of the plates used is concerned, the effects are not very remarkable with a thickness of 10 cm., but become so at 15 cm., and up to 35 or 40 cm. If two plates of 20 cm. each, with grain at right angles to one another, be interposed, the spark is extinguished.

Wooden plates, therefore, cut parallel to the grain behave towards electric radiation as do plates of crystal cut parallel to their optical axis towards rays of light. Further researches made with plates of wood cut at right angles to the grain gave as result a simple refraction, as was to be guessed from the optical analogy. Other woods were also tried, beech being almost as good as spruce, and oak somewhat weaker in its action.

S. P. THOMPSON and MILES WALKER—MIRRORS OF MAGNETISM.

(*Philosophical Magazine*, *Vol.* 39, *No.* 237, *p.* 213.)

The authors point out in this paper the points of resemblance between the magnetic effect of an iron plate placed near a coil or solenoid having a magnetising force, and the optical effect of a mirror similarly placed. They point out that electrical images have long been utilised to elucidate difficult problems in electro-statics, but they think that magnetic images are likely to prove of more practical value.

If a solenoid of wire through which a current is passing be placed against a large plate of iron, the effect of that end upon the distribution of the field about the coil is altered in the same manner as it would be if the coil were twice as long; while, if a plate of iron be placed against each end of the coil, the distribution becomes the same as if the coil were infinitely long. If a mirror be substituted for the iron plate, analogous optical effects are observed. The optical effects can only be obtained perfectly from a perfect mirror of infinite dimensions; and, similarly, the magnetic effects require an infinitely large and perfectly permeable plate to be realised in perfection; and both only take place on the solenoid side of the mirror or plate.

The authors have tried to ascertain to what extent the effects indicated above are realised in practice, and wound two solenoids, each 5 cm. long and of 4 cm. mean diameter, and a search coil of 100 turns about 1½ cm. in diameter was mounted in such a manner that its position with regard to one of the solenoids could be varied. The field in the search coil was then observed first with the solenoid on the iron plate, and then on the other solenoid; the position of the search coil being varied along the axis of the solenoid from the place where it touched the plate to a point about 5 cm. away from the top of the solenoid. The following table, showing the results for various positions of search coil, shows how perfectly the plate acts as a mirror of magnetism:—

Position.	Throw when Coil on Coil.		Throw when Coil on Iron.	
	Make.	Break.	Make.	Break.
1	236	236	236	235
2	235	235	234	234
3	230	230	230	229
4	227	227	228	227
5	204	202	204	202
6	129	126	129	127
7	69	67	69	68
8	41	41	41	41
9	24	24	24	23

The experiment was then made of lifting the solenoid by steps a little distance from the iron plate, with the result that an image was produced which receded by an equal amount behind the plate. A search coil placed in any other position with regard to the solenoid confirmed the above observations. The effect of tilting the coil at various angles to the plate was then observed, the plate being replaced by a frame carrying the other solenoid at a similar, but opposite, angle; with the result that for all angles from 0° to 90° the iron plate acted as a mirror within limits not greater than those in the above table.

It should be noted that the plate acts as a reflector of the current in the solenoid, and not of its magnetic polarity, the reflection of a N. pole being a S. pole, and *vice versâ*. The image of a current flowing in any direction in a plane parallel to the mirror, is another current flowing parallel to the first and towards the same part.

Suggestions as to the effect of inclined plates in producing kaleidoscopic magnetic effects, and considerations of the theory of curved reflectors, conclude the paper.

F. HIMSTEDT—AN ABSOLUTE MEASUREMENT OF RESISTANCE.

(*Wiedemann's Annalen, Vol.* 54, *No.* 2, *p.* 303.)

The author has made a new determination of the ohm in terms of the Siemens mercury unit by his inductive method, and has obtained a mean result—

$$1 \text{ ohm} = 1{\cdot}06282 \text{ Siemens units.}$$

E. LECHER—A STUDY OF UNIPOLAR INDUCTION.

(*Wiedemann's Annalen, Vol.* 54, *No.* 2, *p.* 276.)

The article seeks to determine the question whether in a cylindrical magnet rotating on its axis the lines of force rotate also, or remain stationary.

Faraday, contrary to the statement of his views put forward by some writers, was of opinion that a rotating magnet does not rotate its field with it; and the author quotes passages from Faraday's works to show that this was so.

The field of force, says the author, consists in some condition of the ether brought about by the magnet. It is not necessary to assume that every magnet, when moved, moves all the ether within the range covered by the forces due to it; we can also imagine that the ether stops where it is, and that when the magnet is moved, that alteration of the condition of the ether which we call a magnetic field attacks a new piece of the ether. By a motion of translation of the magnet, therefore, the magnetic lines of force are not actually, but only figuratively, moved. If we move a point of light, the rays of light also only move with it in a figurative sense, as new rays are starting at every moment from the places successively occupied by the point of light during the motion.

When a symmetrical magnet rotates on its axis, the outside field remains unaltered, and there is *à priori* no ground for imagining that this field rotates at the same speed as the magnet: it might be conceivable that the field itself was a rotary phenomenon, in which case its rotation would have a speed independent of the rotation or non-rotation of the magnet itself. In opposition to Faraday's view, Preston, supported by Weber and Lord Rayleigh, comes to the opposite conclusion; and, though the author gives reasons for disagreeing with him, he points out that Preston is correct in saying that Faraday never succeeded in devising an experiment to prove his statements. All experiments with a single rotating magnet are inconclusive. For instance, those of Hoppe—in which an iron tube is magnetised by a fixed coil of wire—can be, and are in the paper, shown to be consistent with both explanations; as are also those of Weber, in which a current is made to rotate round a fixed magnet. After detailing certain experiments with the electrometer, which rest on the deduction of Faraday that a magnet so rotated shows electrical charges, but which led to no results which can be considered decisive, the author proceeds to a section dealing with experiments which he considers of more value.

A magnet is symmetrically divided at its equator by a plane section perpendicular to the axis, and each portion is capable of independent rotation. The gap between the two parts was 9 mm., as the support between the two had to have a certain rigidity, and the length of each portion was 16 cm. The wire coils rotated with the magnet, the current being led in by a rubbing contact on each, insulated from the core. Five terminals—*a*, *b*, *c*, *d*, *e*—serve for experimenting on the unipolar induction; *b* and *d* lead to two rubbing contacts on the equator of each magnet, metallically connected to the core and insulated from the windings; *c* is connected to both axes, *a* with one of the axes, and *e* with the other. *c* and *d* are connected to the galvanometer, and the magnet to which *d* is connected is slowly rotated while the other is still. After making all corrections, the deflection is 38. If now *d* and *e* are connected to the galvanometer, the deflection is 39. In both cases the current flows from the equator to the axis (or, if the direction of rotation be reversed, or the direction of the magnetisation, from the axis to the equator). If the other magnet be rotated as well, the result is identically the same. The equality of the two deflections is not easily explained, says the author, on Preston's theory. Are the lines of force between the magnets stuck to the rotating or the fixed magnet?

Further, attach *b* and *c* to the galvanometer. Rotate magnet I. and keep II. still: the current is 40—almost the same as in the opposite case, as above. Now connect *b* and *d*, the two equators, to the galvanometer: if magnet I. rotates and II. be still, the deflection is 40; and if II. rotates and I. be still, the deflection is 38. But now, if both magnets rotate in opposite directions, the deflection is 79, or very nearly 38 + 40. This remarkable result is easily explained on the Faraday hypothesis, for both magnets are cutting the fixed induction in opposite directions. To explain it on the Preston theory leads to the strained assumptions (*a*) that the lines of force whose rotation causes the phenomena of unipolar induction, and those which produce the ordinary outside phenomena, differ from

one another, and (*b*) that the presence of a second magnet near the rotating one causes an increase in the lines of force, which then proceed to rotate independently of this second magnet. But the author considers that even these inconceivable assumptions are rendered unlikelier by the following main experiment:—

The pivots of the magnets on the dividing support are of ebonite, and to the end surfaces of the poles copper plates are attached, slightly larger than the polar surface, and dipping into two mercury troughs. Whereas, therefore, in the former case the two axes were electrically connected, they are now only connected through the copper plates and the mercury troughs; the latter being metallically connected together and to the terminal *c*. The following results were obtained:—Galvanometer on *a* and *b*, deflection 40, whether magnet II. rotates with I. or not; galvanometer on *c* and *b*, deflection 7 in the opposite direction, also independent of II.'s rotation; galvanometer on *a* and *c*, deflection 32—the difference between the above. The different results from the former ones are due to the fact that the copper plate rotates with the magnet, and the currents, according to Faraday, are due to the lines of force coming out of the cylindrical surface of the magnet.

A further analogous experiment is as follows:—*a* and *e* are connected to the galvanometer, and the conductors to it run in a line following the magnetic axis. Now, if I. alone rotate, the deflection is 33; if II. alone rotate in the opposite direction, the deflection is 33·5 in the same direction. If both magnets rotate in opposite directions, the deflection is 66; if they rotate in similar direction, there is no deflection. While this is easily explained on the Faraday hypothesis, with that of Preston it seems inexplicable. The only fixed conductor cut by the induction of the rotating magnet is the surface of the other, and there is not nearly enough induction there to explain a deflection of 33.

Finally, the author says that the experiments above detailed point strongly to the conclusion that the lines of force stand still when the magnet rotates, and that Faraday's first opinion was correct; from which the following consequences result:—The earth, a magnet rotating in its own field of force, should be charged positively at the north pole, and negatively at the equator. Again, every conductor rotating with the earth must have potential differences in it, indistinguishable by ordinary methods, because a closed circuit is necessary, in which the effects are balanced.

E. TAYLOR JONES—ON ELECTRO-MAGNETIC STRESS.

(*Philosophical Magazine, Vol.* 39, *No.* 238, *p.* 254.)

The author's experiments were conducted with the object of ascertaining the relation between induction and tractive force in magnets, and to what extent this relation was in accordance with Maxwell's theory that the pull should be proportional to the square of the induction.

The disagreement of late experimenters is commented on; Bosanquet's results of 1886 being very discordant, and those of Threlfall in 1894 being equally

unreliable, and only ranging from 11,000 to 16,000 C.G.S. lines. The author used an ellipsoid of revolution of length 22·57 cm. and least diameter 1·5 cm. its volume very carefully measured, and cut through in the equatorial plane; the diminution of length being measured by observing the distance between two marks on its surface. These two halves were magnetised by similar coils, being soldered into tubes which fitted tightly axially into the coils, the free ends projecting by amounts regulated by nuts running on the tubes. To secure good alignment of the half ellipsoids, a brass ring 1 mm. wide was made to fit tightly on one half and loosely on the other at the plane of contact. The use of this ring was found to be essential for consistent results. The weight supported by the lower half was measured by a scale-pan and weights; the magnetisation for given currents was determined by magnetometer, giving the relation between I and H, allowance being made for the demagnetising force of the ellipsoid calculated from Maxwell's formula; and from this curve B could be determined by the relation $B = H + 4\pi I$. Each current was reversed, and half the sum of the deflections, which were generally the same, was taken as the correct value. The lifting experiments were carefully performed for currents ranging from 1 to 10 amperes, the corresponding inductions being 6,000 to 20,000 C.G.S. units; the adjustment of the upper coil being made between each reading, and the surfaces carefully cleaned.

The first experiments were made with plane but unpolished surfaces. The attraction of the coils was found to be never more than one-sixth per cent. of that of the magnets, and could be neglected. The results did not agree with Maxwell's theory, showing greater attraction for low values and less for higher values of induction. The surfaces were then polished, and a better guide provided for the lower coil, and the new curve proved much nearer to Maxwell's formula; the author attributing this result to the better guidance. It was found that at low inductions, by very slightly altering the position of the upper coil, a position of minimum tractive force could be found, in which the coil was in unstable equilibrium, and the measurements were therefore made as follows:—The current being kept constant, shot was poured into the scale-pan in instalments, the upper coil being carefully moved by hand until the minimum position (easily observed by the jerk) was found; and this was repeated until the coil fell in passing over this position. In Threlfall's paper it is shown that at small inductions the tractive force is less when the pole-faces are in contact than when they are separated by a small amount at one side. This was the case in the author's experiments up to $B = 14{,}000$.

The first set of experiments under these conditions gave weights uniformly too low, the deviations from the theoretical value increasing to 3 per cent. at $B = 20{,}000$. This was found to be due to an error in the calculation of the induction in the gap, which was taken too great, as the leakage due to the magnetising coils being 5 mm. apart was not allowed for. The distance was reduced to 1·5 mm., and the results then agreed with Maxwell's theory within one-half per cent. up to $B = 19{,}000$, and within one per cent. up to $B = 20{,}000$. The following table gives the value of B, the square root of the observed tractive force in grammes, and the calculated value of square root of the tractive force. Each

value of tractive force is the mean of several observations, which never differed by more than one or two per cent.

Induction. C.G.S. Units.	$\sqrt{\text{Observed}}$ Grammes.	$\sqrt{\text{Calculated}}$ Grammes.
6,198	52·10	52 06
6,929	59·05	58·40
8,122	68·61	68·55
10,726	90·86	90·59
12,517	105·00	105·60
14,635	122·30	123·50
16,261	136·80	137 30
16,975	142·60	143·20
17,690	148·7	149·30
18,545	155·8	156·50
19,729	164·2	166·60
20,234	168·4	170·80

A. LOHMANN—THE SLAVIANOFF ELECTRIC CASTING PROCESS.

(*Elektrotechnische Zeitschrift*, 1895, *No.* 22, *p.* 325.)

Slavianoff avails himself of the heat produced by the electric arc, as Benardos does in his system of electric welding. The process consists chiefly in pouring the metal which has been melted by means of an electric current on to the surface of any metallic body, portions of which become also molten, and become so intimately mixed with that poured on as to form practically one piece. The main idea is the utilisation of two electrodes of metal, one of which serves as the material for the casting. The other electrode differs widely in different cases. The electrode forming the casting material is in the form of a bar, and quickly melts away under the action of the arc, and the molten metal is allowed to drop into a receptacle which forms the second pole. It is not allowed to cool immediately, but a vessel of molten metal is secured by keeping the arc playing on the surface, and the amount of molten metal obtainable depends on the current. The first difficulty arises in connection with the regulation of the arc, especially in starting, when touching the poles together will often make them adhere, and the difficulties of "feeding" are also very great; and to avoid these, Mr. Slavianoff has devised an automatic regulator, fully described and illustrated in the paper, and which regulates the hand-feeding of the workman in such a manner that all small irregularities are wiped out. The fact that twice as much heat is developed at the positive as at the negative pole is also utilised to concentrate the heat on the material to be cast, or *vice versâ*, while with cast iron the quality of the casting is affected by the sign of the pole.

The current used is about 7½–8 amperes per square millimetre of sectional area, or about 600 amperes for a centimetre bar; the volts being always about 50–70. The power necessary for melting a centimetre bar is therefore about 36 units, while for a 6-mm. bar about 14 units are required. Bars smaller than this are not used, as the amount of molten metal procurable in given time becomes too small, and the amount of heat given off does not suffice to keep any depth of molten metal in the liquid condition.

The Slavianoff regulator enables an ordinary dynamo to be used, care being taken that the sudden variations of load when putting on, say, 600 amperes are not calculated to strain the machine. The firm of Pintsch, of Berlin, who hold the license for Germany, have used a direct-coupled wheel armature machine for 2½ years without any breakdown.

The author then details the application of the process, beginning with the statement that the method is naturally more expensive than the ordinary one, but the cases in which it finds application are of such a nature that the cost is not of primary importance.

1. Small articles can be cast in the following manner:—In a graphite crucible, a rod of the material to be cast is placed in such a position that the arc takes place just on the bottom of the crucible. The other pole is worked vertically by the regulator. As the metal melts, the vertical rod is raised and keeps the arc playing on the surface, the other rod lying in the molten metal, and gradually itself melting away. By using rods of different metals any desired alloy may be produced.
2. Cracks in any metal object can be repaired.
3. Broken pieces of machinery, such as shafts, can be melted together again.
4. Porosities of all kinds in cast iron and cast steel, as well as copper and bronze, can be filled up without any serious difficulty.
5. Worn surfaces can be repaired by melting on a new piece and re-surfacing.
6. Different metals can be melted together into one piece—cast iron to steel, copper and bronze to cast iron, &c. A layer of hard cast iron can be superposed on a piece of softer quality. Such work is useful where it is necessary to lower the coefficient of friction, or to produce surfaces of greater durability than the mass of the material.
7. Holes which through alteration of design are no longer required, or have been made by mistake in the wrong place, can be filled up.
8. Bad welds can be improved.
9. The change of the hard white quality into soft grey cast iron is a special feature of the Slavianoff process.

Details and illustrations of repairs to broken cylinders, fly-wheels, and so on, are given in the paper, as well as the general mode of procedure for casting different metals. The immense utility of such a process for repairs is well illustrated by the case of a fly-wheel given in the paper. The wheel was 7 feet in diameter, and weighed 2,000 lbs., and had been working several years. In alterations to the engine it had to be drilled for a larger shaft, and in placing on the drilling machine was accidentally dropped, and its rim was broken. There was no pattern at hand, and delivery was very urgent. In the space of 24 hours

the repairs were completed, by insertion of a new piece in the broken portion, at a total cost of 15s.

G. GEORGES—A COMPARISON BETWEEN MONOPHASE AND POLYPHASE CURRENTS.

(L'Éclairage Électrique, Vol. 1, 1895, No. 11, p. 512.)

The following table gives the results of calculation to obtain a relation between monophase and polyphase alternating currents :—

——	P. Polyphase Currents.	M. Monophase Currents.	Ratio. $\frac{M}{P}$.
Torque	$C_1 M x^2 (1 - v)$	$C_1 M x^2 \frac{(1 - v^2) v}{2}$	$\frac{(1 + v) v}{2}$
C^2 R losses in armature	$C_2 M x^2 (1 - v)^2$	$C_2 M x^2 (1 - v^2)^2$	$\frac{(1 + v)^2}{2}$

C_1 and C_2 are constants depending on the dimensions and on the frequency of the motor.

If, for instance, one assumes a 5 per cent. slip, then $v = 0{\cdot}95$, which would give 0·925 for the ratio of the torques, and 1·90 for the ratio of the losses. A polyphase motor working with a 5 per cent. slip and a 5 per cent. loss in the armature conductors would, if working monophase, have 9·5 per cent. loss in the armature conductors, and its torque would be 7½ per cent. less than when working polyphase. The above considerations assume no magnetic leakage, but as in practice this would be greater in the monophase motor, its power would be still further diminished. The maximum torque is also much lower in the case of monophase motors, which consequently renders them liable to pull up with a not very considerable increase of the normal load. A two- or three-phase motor worked from a single-phase current has, for equal power, twice as much loss in the armature as when working under its normal conditions. In actual practice it is found that a monophase motor gives about 70 per cent. of the output of a polyphase motor of the same weight and price, and that its efficiency is 6 to 10 per cent. lower.

The advantages obtainable with polyphase motors become most evident in the case of small motors, where heating is an important consideration. The two- or three-phase systems offer about the same advantages, except in the case of high tension, where it is preferable to employ the latter.

With regard to the consumer, it is immaterial which of the two systems be employed. He benefits by the polyphase system when motive power is employed.

The objections which have been raised against the regulation of the three-wire system, do not exist so long as the machines are designed to have a small armature reaction. In fact, when a three-phase motor is switched on, the current taken is not only smaller, but is divided between three circuits, which, of course, is not the case where a single-phase is employed. Indeed, the motors have a compensating effect, for when the voltage of one branch is lower than that of the others, the motor takes less current from it than from the others; in cases of

large differences of voltage the motor can even transmit electrical energy from one branch to the other.

In the three-phase system adopted by M. Ulbricht at Dresden, star coupling is employed; all the lamps are grouped on one side, and the third wire is used for running motors.

The author concludes with the following remarks:—

1. The three-phase system is more economical than the other systems, especially in the case of transmission over long distances.
2. The regulation with three phases presents no difficulties.
3. Polyphase motors are less costly, more efficient, easier to manipulate, and produce a lower drop in potential than monophase motors.

P. HOHO—THE USE OF TWO OR THREE MOTORS ON ELECTRIC CARS OR LOCOMOTIVES.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 3, *p.* 402.)

In practice it is sometimes found necessary to employ two or more motors on an electric locomotive.

The author shows that this use of more than one motor on a locomotive or train not only reduces the efficiency, but has also other serious disadvantages.

In every case each motor either drives one or two axles. If R_1 and R_2 be the total rolling friction, and Q_1 and Q_2 the friction due to slipping, for the first and second motors respectively, although at some moments these quantities may be equal, at others they will differ very widely, depending on the distribution of the load, and on variations in the nature of the track.

The case is first considered in which the two motors are coupled in series.

The force exerted on the periphery of the armature is K I H. K is a constant, I the strength of current, and H is the strength of the magnetic field; these values being equal in the two motors.

If the resistances to be overcome by the two motors be equal, then

$$K\,I\,H = R_1 + R_2\,;$$

but if $R_1 < R_2$, then the first motor will tend to run faster and the second one to run slower. But the speed of the two motors must be the same, as they are mechanically coupled. The resistances offered to the two motors will then become $R_1' = R_2'$ $\frac{R_1 + R_2}{2}$ K I H. This will produce slipping in the driving gear or between rails and wheels.

Under these conditions one motor will be taking nearly the whole E.M.F.—$E = (R + 2\,r)\,I$—and spending its energy on overcoming the resistances due to slipping. E = total E.M.F., r = resistance of each motor, R = resistance outside the motor.

It is therefore evident that two or three motors of N H.P. would give less useful power than one motor of 2 *n* or 3 *n* H.P.; and it has been found in actual practice that under certain conditions the use of two motors, instead of one of twice the output, decreased the efficiency by 60 per cent. It has been assumed

above that the fields of the two motors are equal, which is, however, not strictly so in practice; and this would still further reduce the efficiency.

When the two motors are coupled in parallel they then become more independent of one another. To overcome the resistance R_1 and R_2, the motor will take currents $K_1 H_1 I_1 = R_1$, and $K_2 H_2 I_2 = R_2$. The speeds of the motors will tend to differ according to the values of R_1 and R_2. But, as these must necessarily be the same, the two motors will therefore exert the same force, and the resistance to be overcome will be divided between the two motors—

$$R_1' = R_2' = \frac{R_1 + R_2}{2}.$$

This result is the same as when the motors are coupled in series, but the causes in the two cases are just opposite.

When the motors are compound wound they then combine the advantages and disadvantages of both systems.

The author concludes by remarking that the use of two or more motors on a locomotive or train offers great disadvantages with regard to slipping. If in some cases it is necessary, from practical considerations, to divide the power between two or more motors, these should then be coupled in parallel.

H. GEORGES—THE COMPARATIVE COST OF MONOPHASE AND POLYPHASE ALTERNATING-CURRENT SYSTEMS.

(*L'Éclairage Électrique, Vol.* 1, 1895, *No.* 10, *p.* 462.)

Alternating-current systems may be subdivided as follows:—

Monophase Currents—

1. Two-wire system.
2. Three-wire system.
3. System with individual transformers.

Polyphase Currents—

1. Triphase currents (*a*) with star or triangle connections; (*b*) star connections with fourth conductor.
2. Biphase currents (*a*) with four wires; (*b*) with three wires.

The polyphase system is the more economical, owing to the fact that the circuits can be coupled up together, thus reducing the current-density, and consequently the losses. For instance, when the two circuits of a two-phase system are coupled together, the current is not doubled, but multiplied only by $\sqrt{2}$.

If the resistance of the four conductors be R, and that of the three conductors R_1, then

$$2\, I^2 R = (\sqrt{2}\, I)^2\, R_1;$$
$$R = R_1.$$

It is therefore possible to economise one wire without increasing the loss in the conductors.

In the case of generators and motors it may be assumed for purposes of

comparison that, all other things being equal, armature reaction and magnetic leakage are proportional to the strength of current.

For equal powers from any given design, the following are the ratios of the current-densities with the three systems :—

Monophase.		Biphase.		Triphase.
1		$\frac{1}{2}$		$\frac{1}{\sqrt{3}}$
100	...	50	...	57·8

In machines where the armature consists of a ring of iron without polar projections, and when the winding is uniformly distributed over the whole periphery, the following are the ratios of the powers obtained with the same induction in the iron :—

Monophase.		Biphase.		Triphase.
66·7	...	94·5	...	100

These ratios are as the peripheries of 2-, 4-, and 6-sided polygons inscribed in the same circle. With alternating currents it is, however, not always advisable to completely wind over the whole periphery, on account of the opposing effect produced by some of the turns.

If E is the E.M.F. obtained with one pair of coils, then the power obtained with monophase winding $= P_M = 2\ E\ I \cos 30^\circ = \sqrt{3}\ E\ I$; with three-phase winding the power would be $P_T = 3\ E\ I$. The ratio between these two = 0·578, as given above. If the current be then increased to produce the same loss, the ratio then becomes $\frac{P_M}{P_T} = \frac{\sqrt{2}}{2} = 0{\cdot}707$. Under these conditions, however, the armature reactions with the single-phase would be much greater than with the monophase current.

With respect to the weights of copper in the line, for equal losses and voltage these work out as follows :—

Monophase.			Biphase.			Triphase.	
2-wire.	3-wire.		4-wire.	3-wire.		3-wire.	4-wire.
100	31·25	...	100	72·8	...	75	29·2

These figures apply only to the low-tension circuit, where insulation does not enter into consideration, and where the lamp voltage forms the chief consideration. In the case of the high-tension circuit it is necessary to consider the pressure which may exist between two conductors. In the two-phase system with three wires the pressure between the outside wires will be $\sqrt{2}$ times greater than the normal pressure. Under these conditions a normal pressure of 1,000 volts would correspond, with regard to insulation, to a normal pressure of 1,400 volts with the monophase or triphase systems. If the pressures be reduced to equivalent values, the relative weights for equal losses and power will therefore work out to—

Monophase.		Triphase.		Biphase.	
2 wires.		3 wires.		4 wires.	3 wires.
100	...	75	...	100	145·5

With respect to the installation of the line, the cost is increased with polyphase currents, owing to the increased number of insulators and fittings. The cost of transformers is also greater than with monophase currents.

A. BLONDEL—THE DIRECT MEASUREMENT OF THE MEAN SPHERICAL INTENSITIES OF SOURCES OF LIGHT.

(*Comptes Rendus*, *Vol.* 120, *No.* 10, *p.* 550.)

The system hitherto employed for measuring the mean spherical intensity of a source of light, due to M. Allard, is difficult to perform, and not accurate when the source of light is unstable, such as is the case with the electric arc. By the graphical method originated by the author these measurements can be made with great rapidity. The apparatus consists of a photometer, a diffusing screen, and a reflecting mirror, in front of which is placed the source of light. The accuracy of this instrument requires that the coefficient of reflection of the silvered mirror should be the same at all points; but, as this coefficient varies with the angle of incidence, the latter is made as constant as possible by reducing the mirror to a zone of revolution round the optical axis. The source of light is surrounded by an opaque sphere, made perfectly black inside, and having an aperture at each side. The light passing through the two apertures is reflected from the mirror on to the photometric scale. The mirror forms an ellipsoidal zone of revolution, having as foci the centre of the hollow sphere, and the screen at 3 metres distance, and on which a small spot of light is obtained. As the angle of the beam of light is only 18°, it becomes necessary to take two readings for points at right angles to one another. A great advantage with this instrument is that all stray light is cut off.

The author calls this instrument a "Lumen-Meter," and with it either comparative or absolute measures can be made.

The instrument can be calibrated by employing a standard of known horizontal intensity I h. The apertures on each side must be reduced so that the intensity emitted on each side shall be quite uniform. If s be the solid angle bounded by the aperture, and I the intensity of the spot on the screen, then the constant of the instrument is $K = \frac{I}{s\,I\,h}$. By this means direct readings can be obtained with the photometer.

A. D'ARSONVAL—A NEW METHOD OF ELECTRIFYING THE HUMAN BODY: A MEASURE OF MAGNETIC FIELDS OF HIGH FREQUENCY.

(*Journal de Physique*, *Vol.* 4, *March*, 1895, *p.* 133.)

If a person be placed in an oscillating magnetic field of very high frequency, produced in the interior of a solenoid excited by means of an alternating current, induced currents will be set up in the body, without, however, producing any nervous sensation whatever.

When the solenoid is excited at 15,000 volts by means of a transformer or Rhumkorff coil, one-tenth of an ampere will pass through an incandescent lamp the terminals of which are held in each hand of a person whose arms are placed round the solenoid.

The strength of the magnetic field at such frequencies is measured by means of a small auxiliary solenoid surrounding the bulb of a mercury thermometer.

Foucault currents will be produced in the latter, and its temperature rises by more than 150° in a few seconds.

For lower readings an air or spirit thermometer is employed, the bulb of which encloses a small copper tube.

CH. MARGOT—CURIOUS PHENOMENA OF ADHESION BETWEEN GLASS AND ALUMINIUM AND A FEW OTHER METALS.

(*Journal de Physique, Vol.* 4, *March*, 1895, *p.* 144.)

When aluminium is rubbed on clean glass, a durable trace of aluminium remains fixed to it.

This metallic film is dull in appearance and reflects certain colours, which become more pronounced when the film is polished. When treated with hydrochloric acid or a solution of potash the metal disappears rapidly, but marks are left on the glass as though it had been attacked by being in intimate contact with the aluminium.

This property also exists, but to a lesser degree, with magnesium, cadmium, and zinc.

H. BRUNHES—THE EFFECT OF AN ALTERNATING ELECTROMOTIVE FORCE ON THE CAPILLARY ELECTROMETER.

(*Comptes Rendus, Vol.* 120, *No.* 11, *p.* 613.)

When an electro-motive force of 0·95 volt is applied between the terminals of a Lippmann electrometer, the capillary constant is a maximum. Under these conditions the electrical charges at the two surfaces of mercury and acidulated water are zero, the condenser constituted by the two adjacent surfaces being discharged.

If the electrometer be charged by an auxiliary electro-motive force, the effect will be independent of the sense of this electro-motive force. There should be an effect if the sense of this force varies, or if the sense is alternating.

The author verified this by employing certain electro-motive forces and reversing the sense with respect to the surface of the meniscus by means of a rotating commutator driven by an electro-motor. It was also found that for any other position of the meniscus but that corresponding to the maximum capillary constant, the alternating electro-motive force had no effect.

M. BERTHELOT—ATTEMPTS TO CHEMICALLY COMBINE ARGON.

(*Comptes Rendus, Vol.* 120, *No.* 11, *p.* 581.)

Experiments were carried out on a sample of argon sent to the author by Professor Ramsay. The gas was enclosed in a sealed glass cylinder. The density was 19·95 (H = 1), and the ratio of the specific heats 1·64.

The gas was collected under mercury, and its volume was found to be 37 c.c.

The object of these experiments was to chemically combine argon with other

elements under the influence of an electric silent discharge. This would have a better effect than that of an electric spark, as it would not affect unstable compounds. The action promoted by the silent discharge would be too rapid to produce an ultimate decomposition of products due to an increase in their temperature; an action which would, however, take place with an electric spark. Experiments were consequently carried out to ascertain what effect the electric discharge would have, causing argon to enter into combination with oxygen, hydrogen, and hydrocarbons. Benzine vapour was first experimented with. The action of the discharge on the mixture is accompanied by a faint violet glow, visible in the dark. On one occasion, out of five experiments, there was formed at the end of some time a fluorescent substance, which gave rise to a fine green light possessing a special spectrum. The amount of gas at the author's disposal was too small to allow of his investigating the causes which gave rise to these effects. A second experiment was made with 100 volumes (10 c.c.) of argon placed in contact with a few drops of benzine, and subjected for 10 hours to electric discharges. The benzine vapour was then absorbed by a drop of concentrated sulphuric acid: the residue measured 89 volumes, or a decrease of 11 per cent.

Benzine vapour was next added and more intense discharges employed: the volume decreased more rapidly; at the end of three hours the residue measured 64 volumes.

After a further test the residue measured only 32 volumes. This no longer was pure argon, more than one half consisting of combustible gases.

The analysis of these 32 volumes gave the following composition:—

Hydrogen	13·5
Benzine vapour	1·5
Argon	17·0

The inactivity of argon, therefore, ceases under the above circumstances.

H. PELLAT—ELECTROSTATIC PRINCIPLES WHICH ARE NOT FOUNDED ON COULOMB'S LAWS: THE ELECTRIC FORCE ACTING AT THE SURFACE OF SEPARATION OF TWO DIELECTRICS.

(*L'Éclairage Électrique*, *Vol.* 1, 1895, *No.* 7, *p.* 289.)

Coulomb's law relating to the force acting between two electrified bodies may be stated—

$$f = a\,\frac{m\,m_1}{r^2},$$

where a is a constant depending on the nature of the dielectric.

This relation, however, no longer holds when several dielectrics of different natures (air, paraffin, petroleum), exist between the conductors, owing to the difficulty in giving the correct values to A.

It is therefore not possible to deal with the phenomena which take place at the surfaces of separation of two dielectrics by employing Coulomb's laws.

For investigating the nature of these phenomena the author does not have

recourse to Lord Kelvin's hypotheses on the polarisation of dielectrics, or that of Maxwell dealing with the forces of tension or pressure which the electric field would produce in the dielectric medium. He makes use of certain classical experiments the results of which are undeniable, and also of the laws of thermodynamics. He thus establishes in a general manner, and without making any hypotheses, all the relations which are generally deduced from Coulomb's laws, as well as other hypotheses which were only to be deduced from Maxwell's hypotheses.

M. BERTHELOT—REMARKS ON THE SPECTRUM OF ARGON AND OF THE AURORA BOREALIS.

(*Comptes Rendus, Vol.* 120, *No.* 12, *p.* 662.)

From the results of certain observations, the author suggests that the spectrum bands which characterise the aurora borealis may be due to the presence of argon in some form. It was noticed during certain recent experiments on argon, when in the presence of benzine vapour, and under the action of an electric discharge, that at ordinary pressures the tube containing the gases was filled with a fine fluorescence of a greenish yellow colour, characterised by a spectrum having remarkable lines and bands, resembling that of the aurora borealis, as far as could be seen in the short space of time available during the experiment. Independently of the hydrogen lines and the D lines, numerous yellow, green, blue, and violet lines were observed, the exact position of which was difficult to note. A brilliant line was, however, noticed, placed near the line D, and from which it was separated by a fine black absorption line; and also two groups of bands on each side of D. The appearance of these bands was very similar to that of the group of small lines shown at the left of E in M. Rayet's paper on the spectrum of the aurora borealis. These observations would tend to explain the phenomena of the aurora borealis by the production of a fluorescent derivative of argon.

ANON.—THOFERN ELECTROLYTIC BATH.

(*L'Eclairage Électrique, Vol.* 1, *No.* 12, 1895, *p.* 553.)

In this bath the electrodes are so arranged that the gases produced by electrolysis are obliged to mix together.

The cathode is placed at the bottom of the bath, and the gases produced by it rise to the surface of the liquid, where they meet those coming from the anode.

The bath is circular in shape, and the electrodes are arranged concentrically. The case is fitted with valves to allow the gases to escape, and also with filling and overflow tubes.

The bath also contains a screen, placed between the two electrodes used for regulating the mixing of the gases.

M. HURMUZESCU—THE ELECTRO-MOTIVE FORCES OF MAGNETISATION.

(*Journal de Physique, Vol.* 4, *March*, 1895, *p.* 118.)

In 1881, M. Remsen discovered that magnetised iron is less attacked by acid than when it is not magnetised. Numerous experiments were made at the time to ascertain the relative polarities of the two electrodes, but without any definite results.

The author has endeavoured to ascertain whether there exists an electro-motive force between the two electrodes of the same magnetic metal, differently magnetised, and immersed in the same solution; also the direction of this electro-motive force, and the relation it bears to the strength of the magnetic field.

A capillary electrometer reading to 1-10,000th was employed for measuring the electro-motive forces.

The electrodes consisted of wires 1 millimetre in diameter polished with fine emery paper, and fixed in the two branches of a U tube, one of which was placed between the poles of an electro-magnet. The strength of the magnetic field was measured by the ballistic method. This research was made on iron, nickel, and bismuth, immersed in weak solutions of oxalic or acetic acid.

It was found to be most important that the distilled water employed should be quite pure and free from air, and that everything should be very clean.

The results were found to vary according to the position of the contact of the electrode with the liquid. The electro-motive force of magnetisation is independent of the strength of the magnetic field, of the nature and density of the acid, and of the richness of the salt which is formed.

Iron.—With an iron electrode placed normally to the magnetic field, the magnetised electrode was found to be always positive with respect to the other.

The following are a few of the values obtained, in which the values of the electro-motive force, E, are expressed in tenths of a volt, and the values, H, of the magnetic field are expressed in C.G.S. units:—

E.	H.	E.	H.
5	347	155	3,068
44	1,263	172	3,682
87	2,038	198	4,729
113	2,452	222	6,240

These values plotted as a curve show a point of inflection when H = 2,400.

The same shaped curve was obtained with a great number of different electrodes. The same results were also obtained with an electrode of iron having its axis parallel to the direction of the magnetic field, and coated with an insulating varnish except at a place round the centre.

Nickel.—The results obtained with nickel were analogous, but the curve showed no point of inflection. The electro-motive forces were of the order of a thousandth of a volt for fields of average strength.

Bismuth.—The electro-motive force in this case only reaches a few ten-thousandths of a volt for the strongest fields employed, but the E.M.F. is in the opposite sense—that is to say, the magnetised electrode in the case of bismuth is negative with respect to the non-magnetised electrode.

G. WEISS—A VERY SENSITIVE GALVANOMETER.

(*Comptes Rendus, Vol.* 120, *No.* 13, *p.* 728.)

This instrument is a modification of Thomson's astatic galvanometer, designed for great sensitiveness and reliable working. For a given period of oscillation the sensitiveness of the astatic system is proportional to the ratio of the magnetic moment of the magnets of one pair of coils, to the moment of inertia of the whole system.

In the case of two magnets fixed to a support of negligible mass carrying no mirror, it is evident that the sensitiveness will increase with the shortness of the magnets for an equal magnetic intensity. If one adds to such a system several magnets of the same dimensions, the sensitiveness will remain constant; if a mirror be added, the sensitiveness will increase as long as the moment of inertia of the mirror is not small with regard to that of the magnet. In practice, the advantage of using a large number of small magnets, placed close to one another, is limited by the demagnetising action which they exercise on one another and on neighbouring magnets.

This difficulty may be avoided by forming the astatic system of two long vertical needles placed parallel to the axis of rotation, with opposite poles adjacent to one another, in order to form an almost closed magnetic circuit.

The almost entire absence of demagnetising force allows the induction in the steel to be made as high as desired, and by decreasing the distance between them one can increase the ratio of the magnetic moment to the moment of inertia. In order to reduce the weight of the needles, the distance between the centres of the coils must be made as small as possible; and if the same gauge of wire be used for the whole winding, it is an advantage, independently of all considerations with respect to the astatic system, to make the ratio of the external diameter to the internal diameter smaller than is usually the case.

The author has constructed two galvanometers on the above principle. In the first, the total weight of the movable system was 0·47 gramme. The magnetic system consisted of two pairs of magnetised needles 0·6 mm. diameter and 36 mm. long, their axes being 2·6 mm. apart. The total resistance of the coils was 146 ohms.

If, according to Messrs. Ayrton, Mather, and Sumpner, the sensitiveness of a galvanometer be the number of divisions of deflection produced by 1 micro-ampere on a scale placed at a distance of 2,000 divisions from the mirror, and the period of oscillation be five seconds, then this value for the above instrument was $S = 110$.

The second galvanometer had steel needles 0·2 mm. in diameter and 18 mm. long, with their axes placed at 1·2 mm. apart. The moment of inertia of the system was partly reduced by employing an oblong mirror. The suspension consisted of a spider's thread. The value of S in this case was 1,200; and with a lighter astatic system $S = 1{,}500$.

Although these values are very high for experimental instruments, they would no doubt be considerably increased when special care would be bestowed on their construction.

A great advantage of this system of vertical needles is that the field is very

constant, owing to the almost closed magnetic circuit, as is shown by the following figures:—

11th May,	1894,	1,461.	13th May,	1894,	1,462.
,,	,,	1,457.	,,	,,	1,469.
12th May,	,,	1,467.	,,	,,	1,456.
,,	,,	1,463.	,,	,,	1,459.
,,	,,	1,463.	,,	,,	1,453.
,,	,,	1,458.	,,	,,	1,460.

The above slight differences are attributed to variations in temperature or to errors of observation. The constancy of the field is no doubt partly due to the fact that the magnetisation due to the coils is in a transverse direction, and would therefore have little effect on the vertical magnets.

M. CUNTZ—A SIMPLE EXPERIMENT FOR DEMONSTRATING THE PRESENCE OF ARGON IN ATMOSPHERIC NITROGEN.

(*Comptes Rendus*, *Vol.* 120, *No.* 14, *p.* 777.)

Among the substances absorbing nitrogen MM. Ramsay and Rayleigh have chosen magnesium, in view of the ease with which this metal can be obtained. The complete absorption of the gas is, however, difficult to realise, on account of the high temperature to which it is necessary to raise the magnesium in order to cause it to combine with the nitrogen.

Lithium, which can be prepared in a state of great purity by electrolysis, on the contrary, combines rapidly with nitrogen at a temperature below dull red heat. Owing to this property the presence of argon in atmospheric nitrogen can be easily demonstrated, and also affords an easy method of obtaining this gas.

A small iron vessel containing lithium is placed in a glass tube filled with atmospheric nitrogen, and to which a pressure gauge is connected. When the lithium is gradually heated it becomes incandescent and combines with the nitrogen, producing a partial vacuum; the pressure of the remaining gas corresponding to scarcely 10 mm. of mercury. Atmospheric nitrogen is again admitted and the above operations repeated: the pressure of the residual gas in this case corresponds to about 20 mm. of mercury. After repeating the experiment a sufficient number of times, the glass tube will have become entirely filled with argon.

If the experiment be repeated with nitrogen obtained from nitrogen dioxide, scarcely any residual gas will be obtained; showing that in the above case the residual gas is not due to an incomplete absorption of nitrogen by the lithium.

Argon can therefore be obtained over mercury by causing a current of atmospheric nitrogen to pass through a heated tube containing lithium in sufficient quantities.

DANIEL BERTHELOT—A NEW METHOD OF MEASURING TEMPERATURES.

(*Comptes Rendus*, *Vol.* 120, *No.* 15, *p.* 831.)

Amongst the different methods of measuring high temperatures, the most direct is the one in which the gas thermometer is employed. Although the physical

behaviour of gases depends on relatively simple laws, in the case of the gas thermometer it is necessary to take into account the expansion of the enclosing envelope.

The method devised by the author depends only on certain properties of the gas, and is independent of the shape and dimensions of the envelope. The method is based on the measurement of the index of refraction of the gas.

It has been shown that the refraction $n - 1$ of a gas varies exactly as its density, whether this density be modified by either a change of pressure or by a change of temperature; in other words, although the temperature and pressure may alter, the same index always corresponds to a particular density. This relation is independent of the nature of the function which connects the index with the density. In this method a beam of light is split up into two parts by an interference system, and these are made to traverse two tubes which are filled with the same gas.

The initial position of the fringes is noted. One of the tubes is then raised to the temperature to be measured, the pressure being maintained at that of the atmosphere: the density will diminish, and the fringes will be displaced. The pressure in the second tube is then diminished until the fringes have returned to their original position.

Therefore, since the pressure and density are known in the cold tube, the density is known in the hot tube, and from it the temperature can be deduced This method constitutes the gas thermometer at constant pressure.

A gas thermometer working at constant density might, however, be employed. In this case the fringes would be brought back to their initial position by varying the pressure in the hot tube, and a second tube need not be employed.

One of the chief difficulties experienced in this method was in sufficiently separating the interfering waves to cause them to pass through mediums having very different temperatures.

The accuracy of this method is exemplified by the following experiments made through a range of temperature of 0° to 200° Fah , and with pressures varying from 740 mm. to 763 mm. These experiments were made on the vapours of alcohol, water, and aniline.

Alcohol.			Water.			Aniline.		
Pressure.	Temperature.		Pressure.	Temperature.		Pressure.	Temperature.	
	Observed.	Calculated		Observed.	Calculated		Observed.	Calculated
741·5	77·69	77·64	740 10	99 2	99·26	746·48	183·62	183·54
748·15	77·74	77·86	752·8	99·63	99·74	756·85	183·74	184·07
762·94	78·47	78·36	755·64	99·96	99 84	760·91	184·5	184·28
			761·04	100·01	100·04			

The theoretical boiling points were calculated by the following formulæ :—

Alcohol: $78{\cdot}26 + \frac{H - 760}{29{\cdot}7}$; water: $100 + \frac{H - 760}{27{\cdot}25}$; aniline: $184{\cdot}23 + \frac{H - 760}{19{\cdot}6}$.

In conclusion, this new method allows of the measurement of temperature of a medium by the simple examination of a beam of light which traverses it, and it is quite independent of the thermometric envelope, and is specially adapted for measuring the high temperatures of furnace gases, &c.

A. SADOVSKY—ON THE RESISTANCE OF BISMUTH TO VARIABLE CURRENTS.

(*Journal de Physique, Vol.* 4, *April, p.* 186.)

It is well known that the resistance, C, of bismuth to a constant current increases when under the influence of a magnetic field. The resistance is increased by about 50 per cent. when the value of the field is 12,000 C.G.S. units.

It is probable that a change takes place in the constitution of the metal, for a simultaneous variation is noticed in the specific heat conductivity.

If the current passing through the bismuth is variable, then the resistance has a value O—which is less than C—when the wire is outside the field, or when the direction of the field is parallel to the axis of the wire. But if the axis of the wire be perpendicular to the direction of the field, then the resistance O becomes greater than C for a field of over 6,000 C.G.S. units; the difference O − C then increases fairly rapidly with F, and reaches a value + 0·070 when F = 16,000 C.G.S. units; when placed outside the field it is only − 0·0023.

These effects have hitherto not been explained. They are not due to self-induction, for they are independent of the shape of the bismuth spiral. They cannot be attributed to thermo-electric effects.

The author first studied the effect of varying the number of oscillations of the current; the interruptions being produced by a rotating disc with teeth on its periphery, against which pressed a spring contact. The resistance was measured by Kohlrausch's cylindrical form of Wheatsone bridge, as previously employed by M. Lenard in his researches on the same subject.

The result of these experiments out of the magnetic field was that, if the number of interruptions increase from 100 to 2,000, the value of C − O increases from 0·0012 to 0·0028, C being taken as unity. M. Zahn had previously obtained O − C = − 0·0011; and M. Lenard, operating with 10,000 oscillations per second had obtained O − C = − 0·0023.

When the wire was placed in a magnetic field, it was found that the value O − C still depended on the number of interruptions, N; and, moreover, although the value O − C was about 10 times greater than when out of the field (O − C = 10·02), its variations with N were not more marked than when out of the field. O − C does not therefore become zero for any value of the number of interruptions, N, when placed either in or out of the field.

The author remarks that, according to his experiments, out of the magnetic field, C − O does not appear to diminish when N falls below 400. This leads him to think that N is not the only true variable independent of the phenomenon. If the resistance, O, to an oscillating current differs from the resistance, C, to a constant current, the reason should not be attributed to the reversal of the

current or to the number of oscillations per second, but to the rate of variation, $\frac{\delta i}{\delta t}$, of the current. O may differ from C without the current changing in direction; it is only necessary that it should alter in value. From a reasoning in which he considers the resistance of the conductor during the increase or decrease of the current.

The author draws this conclusion:—

If the observed resistances of a given body to constant or alternating currents are not equal, the resistances in the case of increasing or decreasing currents are not equal. In the case of the oscillating current the resistance of bismuth becomes as $\frac{\delta i}{\delta t}$, a periodic function of the time. It has no definite value, but oscillates with the phase of the current.

After experimenting with sinusoidal currents obtained from a Gramme machine working with a frequency of 3 to 4 periods per second, the author finds that the resistances C with a constant current, C′ with an increasing current, C″ with a maximum current, C‴ with a decreasing current, in the case of bismuth placed in a magnetic field, stand in the following relation to one another:— $C' > C'' > C > C'''$; and every circumstance which increases $\frac{\delta i}{\delta t}$ also increases $C' - C''$.

The following conclusions are arrived at from these experiments:—

1. The difference observed in the resistance of bismuth with constant and alternating currents takes place out of the magnetic field with 300 oscillations per second, and can be observed in the magnetic field with even three to four oscillations per second.
2. This difference depends on the number of oscillations per second of the current, and increases out of the magnetic field at the same rate as the number of oscillations.
4. The resistance which bismuth, placed in an intense magnetic field, presents to an increasing current is greater, and in the case of a decreasing current smaller, than the resistance to a constant current. The difference between these two resistances increases with the rate of variation of the current, and the difference decreases when the initial strength of the current increases.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the months of APRIL and MAY, 1895.

S. denotes a series of articles. I. denotes fully illustrated.

ELECTRIC LIGHTING AND POWER.

G. RICHARD—The Mechanical Applications of Electricity.—*Ecl. El.*, vol. 2, No. 14, p. 13, No. 16, p. 101 (S. I.).

ANON.—Mr. Perry's Article on the Employment of different Methods of Storage of Energy in Central Stations.—*Ibid.*, p. 27.

ANON.—The Pulveriser of the Société Electrique of Crelnhausen.—*Ecl. El.*, vol. 2, No. 15, p. 75.

— L'HOEST—On the choice of Boilers for Electricity Works.—*Ibid.*, p. 78.

— MOISSAN—The Metallurgical Applications of the Electric Furnace.—*Ecl. El.*, vol. 2, No. 17, p. 145.

E. BOISTEL—The Monocyclic System of Distribution of Steinmetz.—*Ibid*, p. 152, vol. 3, No. 19, p. 259 (I.).

W. BOLTON—A Novelty in Electric Lamps.—*Beibl.*, vol. 19, No. 4, p. 346.

ANON.—The Electricity Works of the Town of Salzungen.—*E. T. Z*, 1895, No. 14, p. 196 (I.).

M. KALLMANN—Safety Regulations for Central Station Distribution in the Streets of Berlin.—*Ibid.*, p. 211 (I.).

ANON.—The Electricity Supply Stations of Germany.—*Ibid.*, p. 223.

J. TRUMPY—Service of Electrical Boats at Bergen.—*E. T. Z.*, 1895, No. 16, p. 240 (I.).

E. BOISTEL—Utilisation of the Falls of Niagara: State of the Works.—*Ecl. El.*, vol 3, No. 18, p. 206.

ANON.—The Hardtmuth Arc Lamp Economiser.—*Ibid.*, p. 220.

ANON.—On Direct and Belt Driving.—*Ibid.*, p. 221.

C. E. GUYE—Induction in Armoured Cables.—*Ecl. El.*, vol. 3, No. 20, p. 308.

A. HESS—Distribution of Electric Energy by Polyphase Currents in the Workshops of Weyher and Richemond.—*Ecl. El.*, vol. 3, No. 21, p. 337 (I.).

W. E. AYRTON and E. A. MEDLEY—Tests of Glow Lamps and Description of Measuring Instruments employed.—*Phil. Mag.*, vol. 39, No. 240, p. 389 (I.).

— KITTLER—The Station of the Budapest General Electric Company.—*E. T. Z.* 1895, No. 18, p. 265, No. 20, p. 302, No. 21, p. 314 (I.).

ANON.—Electric Installation Driven by Wind Power.—*Ibid.*, p. 275.

ANON.—Workshop Notes.—*Ibid.*, p. 276.

ANON.—Report of the Darmstadt Electricity Works.—*Ibid.*, p. 280.

ANON.—Proposals for Safety Regulations for Electric Installations.—*E. T. Z.*, 1895, No. 21, p. 319.

A. LOHMANN—The Slavianoff Electric Casting Process.—*E. T. Z.*, 1895, No. 22, p. 325 (I.).

DYNAMO AND MOTOR DESIGN.

A. D. ADAMS—The Best Material for Field Magnets.—*Ecl. El.*, vol. 2, No. 14, p. 31 (I.).

R. PICOU—Design and Construction of Continuous-Current Dynamo Machines.—*Ecl. El.*, vol. 2, No. 16, p. 97; vol. 3, No. 18, p. 202, No. 19, p. 255 (I.).

BEDELL and RYAN—The Method of Operation of the Single-Phase Synchronous Motor.—*E. T. Z.*, 1895, No. 15, p. 228; *Ecl. El.*, vol. 3, No. 20, p. 324.

G. KAPP—On the Predetermination of the Drop in Transformers.—*E. T. Z.*, 1895, No. 17, p. 260.

ANON.—Large Arc Lighting Dynamos.—*Ecl. El.*, vol. 3, No. 20, p. 316 (I.).

ELECTRIC TRACTION.

J. SARCIA—Electric Traction by Accumulators.—*Bull. Soc. Int.*, vol. 12, No. 117, p. 185.

ANON.—The Application of the Three-Wire System to Electric Traction.—*Ecl. El.*, vol. 2, No. 14, p. 29.

— VARNEY—Electric Locomotives in America.—*Ecl. El.*, vol. 2, No. 15, p. 84.

ANON.—Electric Welding of Rails in America.—*Ecl. El.*, vol. 2, No. 17, p. 170 (I.).

Dr. RASCH—Electric Railways and Underground Metal Tubes.—*E. T. Z.*, 1895, No. 14, p. 198 (I.); *Ecl. El.*, vol. 3, No. 18, p. 228.

C. JACQUIN—Electric Traction with Sectional Rail: Claret and Wuilleumier System.—*Ecl. El.*, vol. 3, No. 18, p. 211.

ANON.—Counterpoise Tramways for the Ascent of Inclines.—*Ecl. El.*, vol. 3, No. 19, p. 266 (I.).

G. PELLISSIER—The Electric Tramways of Boston.—*Ecl. El.*, vol. 3, No. 20, p. 295 (I.).

G. KAPP—On Electric Tramways.—*Ecl. El.*, vol. 3, No. 21, p. 371.

ANON.—Tramways in Lausanne.—*E. T. Z.*, 1895, No. 18, p. 276.

A. VOLLER—Disturbances of Magnetic and Electric Measuring Instruments by Electric Traction Current. and its Cure.—*E. T. Z.*, 1895, No. 19, p. 288 (I.).

ANON.—Protection of Pipes against Electric Traction Currents.—*Ibid.*, p. 293.

ANON.—Electric Traction in Cairo.—*E. T. Z.*, 1895., No. 20, p. 209.

MAGNETISM.

A. DE TILLO—Secular Variation of Terrestrial Magnetism.—*C. R.*, vol. 120, No. 14, p. 809.

F. BEDELL—On Magnetic Potential.—*Ecl. El.*, vol. 2, No. 16, p. 133.

E. TAYLOR-JONES—On Magnetic Tractive Force.—*W. A.*, vol. 54, No. 4, p. 641 (I.).

W. SCHMIDT—Researches on the Magnetisation of Iron by very Small Forces.—*Ibid.*, p. 655.

W. WEILER—Unipolar Induction.—*Beibl.*, vol. 19, No. 4, p. 355.

P. CURIE—Magnetic Properties of Bodies at different Temperatures.—*Jour. de Phys.*, May, 1895, p. 197 (I.).

H. PETRINI—Theory of Vector Functions as Ground Work of an Analytical Presentation of the Chief Problems of Electro-Magnetism.—*Beibl.*, vol. 19, No. 5, p. 441.

O. SCHÖNROCK—On the Electro-magnetic Rotation of the Plane of Polarisation of Light in Solutions of Hydrogen Chloride.—*Ibid.*, p. 444.

M. JÜLLIG—On the Form of the Lines of Force of a Magnetic Rotary Field.—*Ibid.*, p. 445.

G. D. E. WEYER—On the Secular Movement of the Points of Convergence of Magnetic Meridians in the last Two Hundred Years.—*Ibid.*, p. 447.

L. A. BAUER—Contribution to the Knowledge of the Secular Variation of the Earth's Magnetism.—*Ibid.*, p. 448.

B. SAUBERT—The Cause of the Earth's Magnetism, and its Importance in connection with Weather Forecasts.—*Ibid.*, p. 448.

INSTRUMENTS AND MEASUREMENTS.

A. SADOVSKY—On the Resistance of Bismuth to a Variable Current.—*Jour. de Phys.*, April, 1895, p. 186.

R. ARNOUX—New Aperiodic Voltmeters and Ammeters.—*Bull. Soc. Int.*, vol. 12, No. 117, p. 165 (I.)

ABRAHAM and LEMOINE—Absolute Electrometer for High Potentials.—*C. R.*, vol. 120, No. 13, p. 176.

P. WEISS—An Extremely Sensitive Galvanometer.—*Ibid.*, p. 178; *Jour de Phys.*, May, 1895, p. 212.

H. PELLAT—New Apparatus for Measuring the Specific Inductive Capacity of Solids and Liquids.—*C. R.*, vol. 120, No. 14, p. 773.

E. BRANLY—Electrical Resistance at the Contact of Two Metals.—*C. R.*, vol. 120, No. 16, p. 869.

J. REYVAL—The Grassot Electricity Meter for Continuous Current.—*Ecl. El.*, vol. 2, No 16, p. 122 (I.).

ANON.—A Pole-Seeker.—*Ibid.*, p. 124 (I.)

ANON.—The Pellat Electric Siren.—*Ibid.*, p. 140 (I.)

ANON.—The Theermann Switch.—*Ecl. El.*, vol. 2, No. 17, p. 169 (I.)

J. KLEMENCIC—An Apparatus for the Demonstration of the Development of Heat in Wires by Electric Oscillations.—*W. A.*, vol. 54, No. 4, p. 760 (I.).

W. OSTWALD—The Chemometer.—*Beibl*, vol. 19, No. 4, p 350.

F. BREISIG—Researches on Induction in Cable Conductors.—*E. T. Z.*, 1895, No. 14, p. 200, No. 16, p. 241.

H. Eisler—The Quadrant Electrometer as a Differential Instrument.—*E. T. Z.*, 1895, No. 17, p. 255.

Anon.—MacEvoy's Electric Submarine Explorer.—*Ecl. El.*, vol. 3, No. 20, p. 323.

W. Hallwachs—On an Aperiodic Quadrant Electrometer, free from Creeping, and having no Magnet.—*W. A.*, vol. 55, No. 5, p. 170.

A. H. Borgesius—An Absolute Electrometer with Mirror and Scale (Duplex Bifilar Electrometer).—*Beibl.*, vol. 19, No. 5, p. 436.

E. Ducretet and L. Lejeune—Portable Apparatus for Rapid Measurements of the Insulation Resistance of Conductors.—*Ibid.*, p. 438.

F. A. Laws—On an Apparatus for the Measurement of the Coefficient of Self-Induction, and on the Phenomena of Alternating Currents. —*Ibid.*, p 444.

E. Mergier—Universal Ohmmeter.—*Beibl.*, vol. 19, No. 5, p. 446.

R. Haas—The Specific Resistance and the Temperature Coefficient of the Copper-Zinc Alloys.—*E. T. Z.*, 1895, No. 18, p. 272 (I.).

L. Kohlfürst—Fiedler's Electric Signal.—*Ibid.*, p. 274 (I.).

A. Prasch—Railway Telegraph Apparatus.—*E T. Z.*, 1895, No. 20, p. 305 (I.).

STATIC AND ATMOSPHERIC ELECTRICITY.

V. Bjerknes—The Penetration of Electric Waves into Metals, and the Electromagnetic Theory of Light.—*Jour. de Phys.*, April, 1895, p. 195.

E. Branly—Loss of Electric Charge of Poor Conductors under Illumination.—*C. R.*, vol. 120, No. 15, p. 829.

Anon.—Apparatus for Testing Lightning Conductors.—*Ecl. El.*, vol. 2, No. 16, p. 131.

A. Garbasso and E. Aschkinass—Refraction and Dispersion of Rays of Electric Energy.—*Ecl. El.*, vol. 2, No. 17, p. 186.

C. Heinke—Studies on Condensers.—*W. A.*, vol. 54, No. 4, p. 577.

L. Graetz and L. Fomm.—On Normal and Abnormal Dispersion of Electric Waves.—*Ibid.*, p. 626.

F. Paschen—Dispersion and Specific Inductive Capacity.—*Ibid.*, p. 668.

W. von Bezold—Remarks on Herr K. Mack's Paper on the Double Refraction of Electric Radiation.—*Ibid.*, p. 752.

L. Lombardi—Slow Polarisation of Dielectrics; Silk as a Dielectric for Condensers.—*Beibl.*, vol. 19, No. 4, p. 342.

R. Arno—On Time Lag in the Polarisation of Dielectrics.—*Ibid.*, p. 343.

D. Mazzotto—On the System of Nodes in Electric Waves produced by Lecher's Method.—*Ibid.*, p. 356.

A. Righi—On Electric Oscillations of Small Wave-Length, and their Application for the Production of Phenomena analogous to the principal Phenomena of Light.—*Ibid.*, p. 357, vol. 3, No. 21, p. 351.

A. Lampetti—On the Determination of the Dielectric Constants by means of Rapid Oscillations.—*Jour. de Phys.*, May, 1825, p. 226.

C. B. THWING—On a Relation between Specific Inductive Capacity and the Chemical Constitution of Dielectric.—*Ibid.*, p. 227.

B. BRUNHES—New Experiments on Double Electric Layers.—*Ecl. El.*, vol. 3, No. 18, p. 193.

— ASCHKINASS—Influence of Electric Waves on the Resistance of Conductors.—*Ecl. El.*, vol. 3, No. 19, p. 283.

E. BRANLY—Remarks on the above Article.—*Ecl. El.*, vol. 3, No. 21, p. 360

V. BJERKNES—On Electric Resonance.—*W. A.*, vol. 55, No. 5, p. 121.

— D'ARSONVAL—New Electrostatic Machine.—*Beibl.*, vol. 19, No. 5, p. 436.

— KLEINER—On a Mechanical Property of a Dielectric.—*Ibid.*, p. 438.

TELEGRAPHY AND TELEPHONY.

E. R. COLLETTE—Long-Distance Telephony.—*Jour. Tel.*, vol. 19, No. 4, p. 73, No. 5, p. 97.

ANON.—Comparative Telegraphic Statistics for 1893.—*Ibid.*, p. 80.

ANON.—Convention for the Establishment of Telephonic Communication between Norway and Sweden, and Sweden and Denmark.—*Ibid.*, p. 88.

A. LACROIX—The Vélophone.—*C. R.*, vol. 120, No. 16, p. 860.

G. DE LA TOUANNE—Notes on Telephony in the United States.—*Ecl. El*, vol. 2, No. 16, p. 125; vol. 3, No. 18, p. 223, No. 19, p. 270 (I.).

E. W. MIX—Telephonic Perturbations due to Alternate-Current Stations.—*Ecl. El.*, vol. 2, No. 17, p. 175; *E. T. Z.*, 1895, No. 14, p. 202.

G. WABNER—Telephonic Systems for Large Towns.—*E. T. Z.*, 1895, No. p. 216.

ANON.—The "Carbonelle" Telephone System.—*E. T. Z.*, 1895, No. 15, p. 232.

V. WIETLISBACH—Note on some Results of the Telephonic Statistics in Switzerland.—*Jour. Tel.*, vol. 19, No. 5, p. 101.

ANON.—Telegraphs and Telephones in the Scandinavian States at the End of the Year 1893.—*Ibid.*, 105.

ANON.—Telegraphs and Telephones in Austria in 1893.—*Ibid.*, p. 109.

J. REYVAL—New Microphone: Salande System.—*Ecl. El.*, vol. 3, No. 21, p. 358 (I.).

ANON.—Telephone Exchange Switches of the Société des Télephones of Antwerp.—*Ibid.*, p. 366 (I.).

ANON.—Regulators and Brakes for the Hughes Instrument.—*E. T. Z.*, 1895, No. 19, p. 292 (I.).

F. BREISIG and B. BOKELMANN—The Current-Curves in Cables when worked by means of Hughes Instruments.—*E. T. Z.*, 1895, No. 21, p. 317 (I.), No. 22, p. 330.

ANON.—Statistics of the Town Telephonic Systems in Germany.—*E. T. Z.*, 1895, No. 22, p. 333.

ELECTRO-CHEMISTRY.

G. SCHMITZ—Experiments with a Carbon Battery.—*Ecl. El.*, vol. 2, No. 15, p. 78.

A C. MACGREGORY—Determination of the Electric Conductivity of certain Saline Solutions.—*Ecl. El.*, vol. 2, No. 16, p. 136.

S. SKINNER—The Clark Cell when producing a Current.—*Phil. Mag.*, vol. 39, No. 239, p. 375.

WARMBRUNN and QUILITZ—New Galvanic Cell.—*Beibl.*, vol. 19, No. 4, p. 345.

G. OPPERMANN—Depolarising Liquid for Galvanic Cells.—*Ibid.*, p. 345.

W. BORCHERS—Experiments on the Utilisation of the Chemical Energy of Carbon as Electricity.—*Ibid.*, p. 346.

F. EXNER—Electro-chemical Researches.—*Ibid.*, p. 347.

S. TANATAR—On the Theory of Electrolytic Dissociation.—*Ibid.*, p. 347.

J. ROSKOWSKI—Studies on Kathode Polarisation.—*Ibid.*, p. 348.

ANON.—A Singular Electrolytic Phenomenon.—*Ecl. El.*, vol. 3, No. 19, p. 263, No. 21, p. 371.

S. SKINNER—The Tin Chromic-Chloride Cell.—*Phil. Mag.*, vol. 39, No. 240, p. 444 (I.).

H. BARNES and E. VEESENMEYER—On the Borchers Gas Cell.—*Beibl.*, vol. 19, No. 5, p. 439.

M. LE BLANC—On the Limits of Electrolysis.—*Ibid.*, p. 440.

H. JAHN and O. SCHÖNROCK—Contributions to the Thermo-Dynamics of Galvanic Polarisation.—*Ibid.*, p, 440.

D. KORDA—On a Thermo-chemical Carbon Cell.—*E. T. Z.*, 1895, No. 18, p. 272.

ACCUMULATORS.

ANON.—Accumulators of 1894.—*Ecl. El.*, vol. 2, No. 15, p. 76.

L. POINCARÉ—On a Class of Secondary Cells.—*Ecl. El.*, vol. 3, No. 19, p 241.

THEORY.

H. POINCARÉ—Remarks on Monsieur Larmor's Theory.—*Ecl. El.*, vol. 2, No. 14, p. 5, vol. 3, No. 20, p. 289.

C. E. GUYE—The Coefficients of Induction of Multiple Conductors.—*Ibid.*, p. 20.

O. COLARD—Equilibrium of a Flexible Wire Conductor in a Magnetic Field.—*Ecl. El.*, vol. 2, No. 15, p. 63, No. 16, p. 112, No. 17, p. 162.

G. N. YULE—On the Passage of an Oscillator Wave-Train through a Plate of Conducting Dielectric.—*Phil. Mag.*, vol. 39, No. 239, p. 309.

H. N. ALLEN—Energy Movements in the Medium separating Electrified or Gravitating Particles.—*Ibid.*, p. 357 (I.).

L. LOMBARDI—Change of Potential and Current in an Open-Circuited Conductor which moves in a Uniform Magnetic Field.—*Beibl.*, vol. 19, No. 4, p. 354.

B. BRUNHES—On the Electro-magnetic Theory of the Absorption of Light in Crystals.—*C. R.*, vol. 120, No. 19, p. 1041.

R. MEWES—The Maxwell Theory of Electro-Magnetism in the Light of the Theory of Vibration.—*Beibl.*, vol. 19, No. 5, p. 447.

VARIOUS.

N. Gréhant—On the Products of Combustion of the Electric Arc.—*C. R.*, vol. 120, No. 15, p. 815.

J. Pionchon—On an Optical Method for the Study of Alternate Currents.—*C. R.*, vol. 120, No. 16, p. 872.

P. Janet—On Eddy-Currents.—*Ecl. El.*, vol. 2, No. 15, p. 49.

A. Blondel—The Determination of the Mean Spherical Intensity of Sources of Illumination.—*Ibid.*, p. 57 (S. I.).

A. Hess—On the Possible Application of Electric Energy to the Production of Organic Compounds.—*Ibid.*, p. 69.

— Gooch—The Hydro-thermo-electric System of Hoho and Legrange for Metallurgical Work. —*Ecl. El.*, vol. 2, No. 17, p. 165.

A. Sadowsky—On some Experiments with Alternating Currents.—*Phil. Mag.*, vol. 39, No. 239, p. 377.

E. Wiedemann and G. C. Schmidt—On Luminescence.—*W. A.*, vol. 54, No. 4, p. 604.

A. Schülke—Plotting the Lines of Force of Electric Currents.—*Beibl.*, vol. 19, No. 4, p. 344.

B. Pierce—Thermo-electric Properties of Platinoid and Manganin.—*Ecl. El.*, vol. 3, No. 18, p. 236.

C. Raveau—Annual Exhibition of the Société Française de Physique.—*Ecl. El.*, vol. 3, No. 20, p. 312.

— Gooch—The Hoho and Lagrange Hydro-thermo-electric System for Metallurgical Purposes.—*Ecl. El.*, vol. 3, No. 21, p, 360.

G. Semenza—The Electrical Industry in London.—*Ibid.*, p. 367.

— Kleiner—The Thermo-electric Behaviour of certain New Combinations of Metals.—*Beibl.*, vol. 19, No. 5, p. 440.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

JOURNAL

OF THE

Institution of Electrical Engineers.

Founded 1871. *Incorporated* 1883.

VOL. XXIV. 1895. No. 119.

The Two Hundred and Eighty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 28th, 1895 — Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on May 23rd were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

The following transfers were announced as having been approved by the Council:—

From the class of Associates to that of Members—

Edgar A. Ashcroft.
John Hesketh.
Joseph Orchiston.

From the class of Students to that of Associates—

Herbert H. Bigland.
Andrew Gray.
Herbert Edward Shreeve.
G. Thomas-Davies.

The Secretary announced that donations to the Library had been received during the recess from the Astronomer Royal; Dr. Bedell; Board of Trade, per Major Cardew, R.E.; Electrical Company, Ltd.; India-Rubber, Gutta-Percha, and Telegraph Works Co.; International Railway Congress; Institution of Civil Engineers; G. C. Maynard, Foreign Member; A. R. Bennett, R. W. Blackwell, Professor Jamieson, *Sir David Salomons, Professor Thompson, Sir Charles Todd, and S. F. Walker, Members; A. B. Chatwood, A. F. Guy, and G. S. Ram, Associates.

On the motion of the PRESIDENT, the thanks of the Institution were unanimously accorded to the donors.

The PRESIDENT: I have now, with much regret, to announce the death, under very melancholy circumstances, of one of our Foreign Members, Mr. Franklin Leonard Pope, an American engineer of considerable eminence, and well known to many members here present. He has upon several occasions attended our meetings, and has, I believe, taken part in our discussions. Mr. Pope was a very prominent member of the American electrical profession, and combined a large technical experience with extensive legal knowledge. He was very highly esteemed and respected, and had held the office of President of the American Institute of Electrical Engineers. The Council propose that a resolution expressive of our deep regret at Mr. Pope's death, and of our sympathy, should be forwarded to the surviving members of his family.

This proposal was unanimously agreed to.

The following paper was then read:—

THE ELECTRIC WIRING QUESTION.

By FRED. BATHURST, Associate.

Mr. Bathurst. The "electric wiring question," which is the subject I venture to bring before you to-night, is one in which each individual member of the electrical profession has directly or indirectly more than a momentary interest.

Moreover, in view of the contemplated issue of a new set of

* Two framed and coloured prints illustrative of railway travelling in 1831.

"Wiring Rules" which will bear the imprimatur of this Institution, it is at the present moment worthy of careful consideration. Mr. Bathurst.

With the information and experience which has been accumulated in the past 12 years, we should now be in a position to lay down rules which will advance the art of wiring towards perfection. Such rules should not only conserve the welfare of the public, but should also regard the interests of the electrical industry.

My object is to ask that, in any consideration of this question, due recognition may be given to the use of an *insulating tube* as one of the factors which must enter into the question of perfect wiring.

Hitherto the ordinances issued in respect to wiring have had for their primary object the prevention of fire, and have, therefore, received the general designation of "Fire Rules." These rules have been rigorously administered under capable, indefatigable, and impartial inspectors connected with the various fire insurance offices. No question can be made as to their efficacy in respect to the prevention of fire, but I submit that, considered from a technical and economical standpoint, they cannot in any way be regarded as ultimate or final. It could be argued with much reason that, from the fact of their having specified definite means and methods under which wiring should be effected, we have even been led to expend our energies in directions which do not truly advance us towards the goal we seek.

Electricity, in respect to its utilisation, should not be tied down to specified lines and methods which hamper its proper development and progress.

The Development and Growth of Wiring.

The electric light, from sheer force of merit, has forced its way into recognition as a luxury, and the public now willingly admit that it is an accessory to their comfort and convenience.

Perhaps I may be pardoned a momentary digression to outline briefly the electrical progress of England as compared with some other country. Let us take the country typical of progress—America.* Both countries were in 1881 on the same

* See Appendix A.

Mr. Bathurst.

basis; the apparatus was crude, experimental, and imperfect, and the knowledge in respect to the safe and proper handling of the apparatus employed was limited, and possessed by but few people. In England in 1883 the possible dangers began to attract attention, and rules and regulations were issued which were to control development and progress. In America no regulations were enforced; for good or evil, a normal development held its course. The American knew his imperfection, but his utilitarian instinct forced him to employ any means at hand by which to gain further experience.

Oftentimes absolute security may have been sacrificed to the necessity of the moment. The American public, however, demanded the new light, and it had to be installed at prices which did not always provide for solidity and permanence. That it was sufficiently satisfactory for acceptance cannot be questioned, since in 1887 they had in operation (not to mention innumerable isolated plants) some 70 central stations, supplying over a quarter of a million incandescent lamps. In England at this time there was *one* central station, supplying about 5,000 lamps, and a proportionate number of independent plants.

It is just here that insurance authorities may ask about electrically caused fires. If so, I seize the opportunity for removing what appears to be a greatly exaggerated apprehension. The hurriedly installed apparatus did cause fires, and in a measure terrorised the public much in the same way as our recent subway explosions in London alarmed the British public. Gradually, also, the American fire insurance offices took alarm; and "Rules" began to see light. Different States had different regulations, and each inspector became a "law unto himself." Ultimately it was seen that "confusion had become worse con-"fused," and that effort must be made to produce rules which would be generally acceptable.

Within the last few years the underwriters determined to appoint an Electrical Bureau to collect information concerning the fires said to be "caused by electricity," in order that some general deductions could be made. The reports issued by the Bureau (under the effective direction of Mr. W. H. Merill, jun.) are most

comprehensive, and give valuable and instructive information in respect to the electrical fire hazard. Mr. Bathurst.

Taking 1893–1894 as two typical years in which public alarm was prevalent in America, reference to the records shows the occurrence yearly of between 400 and 500 fires traced to electrical causes. Some 320 involve no monetary loss, but the remainder (100 to 200) result in a loss of £300,000 per year. This sum, though apparently large in itself, is less than 1 per cent. of the total fire loss of some £35,000,000 from all causes. The bulk of the electrical loss occurs in the Western States, and for the more populous Eastern towns the proportionate loss is less than one-half per cent.

The loss arising from the use of kerosene oil and matches is appalling in comparison, and the figures are best not quoted.

In fact, the insignificance of the danger from commercial electrical application is best shown by comparison with the loss caused by Nature's wayward electrical illumination—lightning. Taking the figures from the United States Government records, I find, from the average monetary loss per year, for nine years ending 1893, that "lightning" involves a loss of £310,000, or some £10,000 per year more than that caused by the commercial use of electricity. It can, therefore, I think, be affirmed that in the country where electrical progress has been least restrained by artificial regulation, the loss on account of the electrical fire hazard is not so formidable as usually supposed.

At the present time America has the benefit of numberless motor applications, and over 8,000,000 arc and incandescent lamps, against, perhaps, 2,500,000 in England.

Can I not, therefore, argue that restrictions of one sort or another have hampered the proper development of our electrical industry?

And, again, is it not a matter of everyday comment with us, that the electrical profession, working under the present conditions, does not reap to the full extent the benefit which should accrue to its work?

Returning to our retrospect, we find, however, that, technically considered, we are no longer in the same position as in 1881.

Mr. Bathurst.

Electrical apparatus in every detail has been the subject of patient improvement. Switches have developed from flimsy, poorly designed mechanisms on wooden bases, to substantial metal and earthenware constructions, which are fire-proof and electrically and mechanically perfect. Lamp-holders, once three-prong wooden devices, fitted with spiral spring contacts, have developed to the present finished type.

The insulation on the wires has changed from paraffined cotton to a durable rubber covering. Not only has the quality of material been enhanced many times, but, with it, market values have fallen so as to be on something like a permanent basis. Central supply stations have been built, and methods of generation and distribution so improved that electricity can be supplied, light for light, at prices directly comparable with gas.

Up to the consumer's premises we have economical electrical production and distribution; and, notwithstanding the expense of the initial installation within the house, a demand for the light has grown. The recent rapid growth of municipally owned electric supply stations would even prove that it is becoming an actual necessity. Why, then, is it that in England, the most prosperous country of the world, this light—the safest and best illuminant—is not making the same rapid headway and progress that it is doing amongst less favoured nations? This, in fact, *is* the "burning question," which I shall endeavour to make as important as that "question of burning" of the fire insurance companies.

Even if it is contended that we are, as a whole, making sufficiently rapid commercial progress, no one will dispute the statement that, commercially considered, the state of affairs with our "wiring contractors" is, to say the least, *bitterly disappointing*. The bare mention of the fact that for the same piece of work tenders may be received which vary in respect to one another by one, two, or even three hundred per cent., is in itself sufficient to show the conditions obtaining in this direction.

The Present Problem.

The principal obstacle to the general use of electrical applica-

tions is that under our present wiring rules it is impossible to place the cost of installation within reach of the great bulk of our people. The largest number of consumers to whom we cater—the middle class—cannot afford the initial outlay necessitated by adherence to our present methods. The introduction of electric lighting has educated them to the point of desiring better and more light, but we still have a further duty to perform in bringing this advantage within their means. Mr. Bathurst.

I submit, therefore, that what is still required is the introduction of rules and regulations which will facilitate the growth of "lower cost wiring." If the term "cheap wiring" is used in this connection, by implication, such work will be termed "nasty." Nothing is further from my intention than to advocate cheap, defective, poor, or dangerous wiring. But, speaking as one whose wish is to promote the welfare of our industry, I suggest that the time has arrived when the advent of "wiring methods" which will reduce the first cost of electrical installation to the general public by perhaps one-half that now obtaining, will, if rightly ordered, give to electrical activity, with its inherent possibilities, that impetus which we all know it to be capable of receiving.

With "lower cost" wiring we could, for instance, successfully compete with the lately introduced "incandescent gas burner," which, priced at about 13s., has had a sale amounting to three-quarters of a million.

It is being recognised that the ruling factor which governs public demand is small initial outlay. Even the gas companies are facing the question of fitting consumers' premises free and reimbursing themselves by charging a slightly increased price for the gas supplied.

The Technical Requirements.

Having established the fact that, either from "excess of "supply" or a "lack in demand," our contractors are face to face with a serious economic problem, it behoves us to look into our present methods of wiring, to see in which direction economic improvement can be contemplated. Let us consider briefly the essential features of good wiring. The five cardinal points may

Mr. Bathurst.

be given as safety, durability, convenience, accessibility, and economy. Professor S. P. Thompson, in a lecture on the subject before the Society of Arts, has embodied the essential features in a few words when he said, "What is wanted is a mode of running "wires and fixing switches and other accessories, so that they shall "not only be electric tight, but shall also be water-tight, gas-"tight, air-tight, oil-tight, fire-tight, and even rat-tight." The only "tight" he has omitted is "money-tight." It is my belief that all these requirements can be met by the use of *fire-proof insulating tubes;* but before dealing with this point I may be permitted to challenge the existing methods in the light of these requirements.

As the advocates of "concentric wiring methods" are, I understand, to present their views, courtesy leads me to avoid any direct criticism of "concentric" systems, except under those general headings in which they could obviously be included.

Present Methods in the Light of the Requirements.

It will probably be admitted that wood casing fails in many respects. As a "mechanical" protection for the wires it is imperfect; and it is as deficient in "insulating" protection, because it is neither damp-proof nor incombustible. To again quote the words of the above-referred-to authority, it may also, like charity, "cover a multitude of sins." In concealed work it is positively an obstacle to the development of electric wiring. It shrinks and warps, causing plaster to crack, and often necessitates costly redecoration. The idea of its use originated with the telegraphic engineer, and 15 years ago electricians were contented with it. Then it served the sufficiency of the moment. On account of its one desirable attribute—accessibility—it may survive somewhat longer for surface work.

Even on the surface, however, it is neither picturesque nor decorative. It is easy to instal, but its nature prevents any very correct or exact estimate being formed of the labour required. If surface work is required on the score of economy, surely it would be better to adopt "open work" on porcelain insulators—a safe and scientific method of wiring, which can be carried out at a very

much lower price. We know this system is discouraged; but it is noteworthy that in the only prominent installation in this country where "open work" has been used, the single source of trouble which occurred arose on a circuit in a damp cellar, where, in order to conform with the fire office "requirements," *wood casing* had been employed. Someone may argue that this could not have occurred if the circuits had been "tested" as the "Rules" recommend. This statement I will answer by a question. It is, whether "insulation resistance" as now specified is not, after all, somewhat of a myth?* Mr. Bathurst.

Our "insulation test" never yet told far into the future, and it only tells the present to a limited extent.

"Insulation test" cannot discover an unsoldered joint or a dangerous diminution in respect to current-carrying capacity. Every practical man will agree that insulation testing will not necessarily indicate that the wires are at places in dangerous proximity to gas or water systems in event of moisture appearing.

To continue, the recognised shortcoming of wood has brought into use iron and "lead compo" piping. Lead compo pipes afford but little mechanical protection in emergency, and, with respect to "accessibility," have serious drawbacks on account of their flexibility.

A large amount of "surface leakage" necessarily arises from the use of any ordinary metal tubing. Moreover, if the insulating material on the conductors has from any reason deteriorated and moisture appears, there will be consequent injury both to conductor and tube. Cases have occurred where the protecting fuses on a circuit did not "blow" before electrolysis promoted a dangerous arc which had burnt a hole in the tube and presented a serious fire risk.

If a defect occurs anywhere in the insulation of a wire which is within a metal pipe so that there is electrical contact between the conductor and the pipe, the effect of the fault is virtually transmitted throughout the whole length of piping, there to await or invite trouble elsewhere. With two conductors in the

---

* See Appendix B

Mr. Bathurst.

pipe a new and unanticipated electrical condition prevails, because one conductor is now of greater carrying capacity than the other, having been increased by the capacity of the tube itself. Overheating of the intact conductor can now invite serious trouble at any point. This, I think, explains why overloaded wires in pipes have occasionally "burnt out" at *several points* at the same instant. It must not be forgotten, also, that failure of insulation on one wire means that the voltage tending to break down the other is doubled, and the actual strain ($C^2 R$) increased fourfold.

Perhaps at this point, already, it will be noticed that my criticisms show, incidentally, how much more our immunity from electrical fires has depended upon the honour and integrity of our contractors than upon the "rules" provided for their guidance.

Iron pipes have the disadvantages common to all metal pipes, and, as we know, may rust inside, and this rust is not a condition favourable to "insulation;" they often have burrs or other internal roughnesses which may cause serious mechanical damage to the insulating material as the wires are being drawn in. If iron pipes are screwed at the end for the purpose of jointing, oil is probably employed, and any excessive presence inside is not helpful to a rubber insulation. Cases are on record where this oil has caused extensive mischief.

Early in the art of wiring it was recognised that gas brackets, when modified so as to serve as "electrics," should be insulated from the remainder of the gas piping; but now that we are using electric wires themselves in metallic pipes, we appear to overlook the possibility of the electric system being elsewhere in hazardous contact with conjointly installed steam or water systems.

It is now apparent that metal pipes, if not actually harmful, are certainly not particularly helpful to the maintenance of insulation resistance.

As thrifty business men we must recognise that when buying electric wires we are paying for *insulation*. It is true we require the copper for current-carrying capacity, but in a high-grade wire the expense of the insulation is about twice that of the copper.

From the commercial aspect, apart from any technical con-

siderations, the cost of a metal pipe is a formidable drawback. The usual clause dealing with the protection of "conductors" says "they must be enclosed in substantial wood casing or *approved* "metal tubes." If this refers to iron or compo pipe, and we are to adhere to the regulation, how can we compete with the gasfitter when we have to supply the pipe as well as the insulated wires inside it? Mr. Bathurst.

The Advantage of Tube or Conduit Methods.

It may be thought that we must turn away from tube or pipe methods altogether in order to find economic wiring. Experience in other countries, however, does not show that any "rigid" system of conductors is making progress or headway. No insulated wire has yet been produced which in itself can successfully resist the deteriorating influences present in mortar or plaster. Even lead-covered insulating wires, if directly embedded in plaster, have been found wanting, and give trouble from "earths" and "short-"circuits" as soon as moisture has made an appearance.

With any rigid method, if walls happen to crack and settle, or plasterers and bricklayers cause damage, a fault may develop which is very difficult to find. Usually, also, they are handicapped commercially in respect to making joints and connections, as these involve risk and trouble even with the highest quality workmen.

A cardinal objection to "rigid" methods is the lack of accessibility. All good wiring should be accessible, for with this quality assured half the difficulty of "trouble" is already fought, and any imperfection or injury, whether arising from poor work, negligence, or accident, can be remedied with a minimum of inconvenience.

A conduit or race-way is the practical way of rendering wires "accessible," for then the wires installed in them are always open to the inspection of owner or wireman, whenever he wishes to satisfy himself as to their condition or to effect a renewal. "Accessibility" ought, therefore, to be a fire office requirement; *

* See Appendix C.

Mr. Bathurst. with it an inspector can use his *eyes*, and have no need to trust to his *ears* for inspection.

From the contractor's point of view "accessibility" is desirable as limiting unfair and injurious competition; for, when an installation can be readily inspected, inferior materials cannot be used for the purpose of cutting down prices, workmanship cannot be scamped, and the "death knell" of unprincipled competition could be sounded for all time.

The Superiority of an Insulating Tube.

This brings us to the consideration of the conduit itself. From a labour-saving point of view a flexible conduit would apparently commend itself. Whilst cases do occur where this is true, every flexible conduit has the inherent drawback, as shown by experience, that the property of "flexibility" may so far assert itself that numberless bends will prevent the wires from being drawn out. Any trouble which then occurs will have to be cut out, or else the circuit must be abandoned or wired anew. Flexible conduits are always at a disadvantage either in respect to moisture-resisting properties, or are wanting in mechanical strength.

Bends and elbows must be *rigid*, or it is impossible to draw the wires in or out; and for the generality of work it will be found that a conduit construction permanently rigid throughout is preferable.

I have already pointed out that more than half of the cost of the wire we use is referable to "insulation," and that it is "insulation" we strive to keep. It would, therefore, appear obvious that the conduit should be an "insulating" one. An ideal insulating tube should not only be mechanically strong, but also fire-proof and water-proof. With a tube complying with these requirements all possible electrical trouble is minimised, the insulation on the conductors themselves is protected from mechanical damage, and is enveloped in a preservative, rather than a destructive, element. Temperature changes are reduced to a minimum, and, if the conduit resists acids, alkalies, and gases, the insulating material on the wire is so shielded that its life is indefinitely prolonged. It

appears to me that the insulating tube does for "wiring" what the high-resistance incandescent lamp did for the "distribution" problem. Mr. Bathurst.

I contend, therefore, that an insulating tube method of wiring will prove the ultimate solution of the "electric wiring question."

THE INTERIOR CONDUIT CO.'S SYSTEM.

Having now hit, as it were, every head within reach, by criticising to the best of my ability the existing systems, I am bound not only to apologise (as I do sincerely) to those whom my criticisms may have aggrieved, but endeavour to make some amends by setting up an idol of my own, which may give opportunity for retaliation.

Accordingly, I have pleasure in showing a few samples of the insulating tubes manufactured by the Interior Conduit Company of New York, in the belief that they represent the highest achievement yet accomplished in the production of the ideal tube. This method of wiring originated with Mr. E. H. Johnson. This name, from its connection with the Edison companies, will be familiar to many. The system and process of manufacturing the tube and accessories have been worked out by Mr. Johnson in conjunction with Mr. Luther Steringer and Mr. E. T. Greenfield as co-patentees. The insulating material employed is a specially prepared hydrocarbon of the bitumen class. The necessary strength in tube form is obtained by using a peculiar grade of fire-proof paper material which admits of impregnation.

The system provides perfect immunity from fire, durability of insulation, accessibility to concealed wires, and reliable service. It has had extensive practical test in America, and more recently in Germany, where its reputation for being the safest, least expensive, and most practical method of wiring is now established. Perhaps the best confirmation I can give of this statement is the fact that we already count, in various countries, some *two million incandescent lamps* installed in the three varieties of tube—"plain," "brass-covered," and "iron-armoured"—and have yet to learn of the first electrical fire arising from its use. Risking the

Mr. Bathurst.

criticism of "advertising," perhaps the best course that I can take is to examine this system in regard to the five cardinal points before mentioned of safety, durability, convenience, accessibility, and economy.

Safety.

We have noted that danger arises from deterioration of the insulating material on the conductors, and that this can be prevented by enclosing the wires in insulating tubes. These insulating tubes, or internally insulated metal tubes, have, therefore, an immediate advantage over plain metal tubes.

Again, when deterioration does take place in a plain metal tube, the tube itself becomes a conductor, and invites deterioration at many points. If insulated tubes are used (with two conductors inside), the deterioration on one only brings additional stress upon the other conductor at one point. The tendency in this case is to create a "short-circuit" at the spot affected. In these particular tubes a short-circuit presents no danger, as the materials of which they are composed will resist the burning effect of the arc. Many tests have been made, and in every case it has been found that the "arc" increased in length until it broke, and severed the circuit without further danger. Any "short-"circuit" trouble, then, removes itself, and its injurious effects are confined entirely within the tube. Since the presence of moisture is the principal cause of "short-circuit," the use of these tubes is likely to minimise the occurrence of trouble, because the tube material is non-conducting in respect to heat, and extraneous changes of temperature fail to produce internal condensation.

The use of insulating tubes will also remove the increased danger in respect of wires employed for heating or power applications, which may be temporarily overloaded, and in respect of wires protected by a fuse which through careless replacement has been made too large.

It may also be that insulating tube methods of wiring will render it possible to materially modify our present practice in respect to "fusing."

While on this question of safety, I should say that there is only one point not yet entirely settled. It is whether the two

insulated conductors should be kept together or separated—whether one or two tubes should be employed. Owing to the real necessity with a wood-casing system of keeping the wires apart, some authorities urge that this construction should be applied to tube methods, and that a separate tube should be used for each wire. Mr. Bathurst.

In alternating-current circuits it is essential to keep the wires close together, to prevent a drop in voltage from inductional effects. Clearly, therefore, in this case both wires should by preference be in the same tube. Since in England two conductors are already allowed in "compo" pipes, I do not here anticipate objection being made to this construction in respect of an insulating tube which is electrically perfect and equal to "compo" mechanically. Moreover, in those positions where mechanical strength is imperative the insulating tube can be supplemented by an iron or steel covering.

I maintain that theory points to the advisability of placing both wires together, and so far, the results of practice favour this view.

DURABILITY.

As far as present experience has gone, it shows that the bitumenised tube will, under ordinary conditions, last indefinitely. It can safely be embedded in the lime and sand plaster used in ordinary building construction. I may mention that the present tendency towards fire-proof building construction, is bringing into use certain cements, caustic enough to attack nearly any material coming into contact with them. The problem thus presented has been successfully solved by the introduction of the "brass-"covered" insulating tube.

The bituminous material used in these insulating tubes is inert in its nature, and exercises no deleterious influence on the insulating material of the wires themselves. It has sufficient mechanical strength to resist any damage except that inflicted by gross carelessness or design, and in the case of chance mechanical injury to the tube the "accessibility" it provides renders repair easy. In respect of permanence and durability, the advantage of placing the conducting wires in channels which

Mr. Bathurst. predetermine their relation to any contiguous conducting bodies, is, I think, beyond question.

The mischief done to wood casing and "compo" pipe by rats and other vermin is, in the case of these tubes, entirely obviated. From the fact that such inroads are successfully resisted, it would appear that bitumen disagrees with a rat's digestive machinery!

Convenience and Accessibility.

I have met some contractors who argue that a "conduit" method is not so convenient as "wood-casing" or "rigid" methods. It is possible this has been their experience from a limited trial. I have no hesitation in stating that when wiremen become even slightly proficient in handling the material, they will prefer to instal "conduits." At least, this has been the universal judgment in other countries.

"Boxes" and "elbows" do not present insuperable difficulties, nor do the "runs," when the rigid conduit can be made temporarily flexible by the use of heat.

A perfect system of wiring should not only give a suitable channel for the wires, but must provide for the convenient jointing of the tubes themselves, and also for "switch" and "lamp" outlets. It should admit of the multiplication of small circuits from convenient "centres of distribution," as this construction avoids the necessity of joints in the wire, and permits equalisation of the voltage throughout the whole installation. An architect ought to be able to lay out the system and provide in his plans for the electrical installation as completely and intelligently as he now provides for steam or gas utilisation. The system I advocate satisfies these requirements, for, after the conduits have been erected and all danger from mechanical injury has passed, the wires can be readily drawn into their respective channels, and connected up to suitable distributing blocks. There are numbers of existing installations where over 1,000 lamps are in use, and in which any wire is "accessible" at any moment.

Economy.

An insulating tube system is also economical. With trained

workmen, we estimate saving from 25 per cent. to 50 per cent. in the labour of installation. Even though the tube in itself is comparatively expensive, if wires suitable to it are employed, the total cost of material will compare favourably with any other system. Mr. Bathurst.

The insulating properties of these tubes is such as to provide (approximately) an additional "300 megohms pro mile" insulation resistance to the wire. We are beginning to appreciate the fact that, although we provide a costly "600-megohm wire," after having installed a few hundred feet our tests indicate a total insulation resistance of only a few thousand ohms. An insulating tube system can enable us to obtain a more commensurable result. If a high insulation test has to be obtained in wiring work, practical experience shows there is little doubt that an insulating tube, and its suitably insulated wire inside, is commercially ahead of a relatively heavily insulated wire.

Perhaps the limit of economy will be reached when we know how to "earth" perfectly, and can use a bare copper conductor within a metal-covered insulating tube.

I might mention that commercial experience in America has led to the gradual development and improvement of the Interior Conduit Co.'s tubes. The plain tube was introduced to compete, price for price, with wood casing, but as it took possession of the wiring field, the manufacturers felt that its one weak point was a lack of full mechanical strength. With the saving it was possible to effect in the labour of installation it was thought desirable to introduce improvement, and the brass-covered tubing became generally adopted. This has been followed by the iron-armoured tube, which is electrically and mechanically perfect, and is now employed for all the highest class work.

It will also keep possible criticism in its true channels if I state that the work done in England up to the present has been tentative, and with a view to find out whether the objection raised by one of the large fire insurance offices to the use of plain tubing can really be maintained. It is certain that good and satisfactory work *can* be done with the plain tube, but I am one of those who would wish to advocate high-class work, and would

Mr. Bathurst. suggest that perhaps iron- or steel-armoured tubing might be used exclusively.

Conclusion.

I have already trespassed too long upon your patience, and would now only express the hope that those—I believe there are several present—who have already used insulating tubes will state their experience.

I have ventured my personal opinion that the insulating tubes before you are the nearest approach yet existing toward the ideal method of wiring, and I trust that the verdict of those who have tried them will support this view.

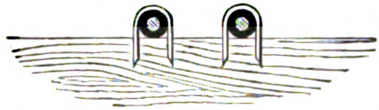
If this be so, I ask that this Institution, in considering "the question of electric wiring," will, in any rules it may issue, give recognition to, and make due provision for, the use of insulating tubes.

Mr. Bathurst.

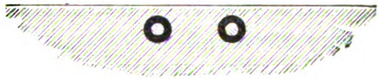
APPENDIX A.

Development of Electric Wiring.

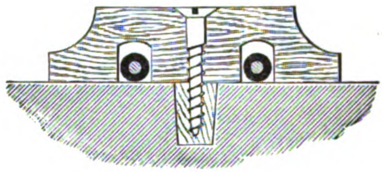
1. Wires secured on the surface by metal staples.



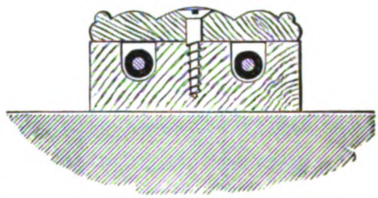
2. Wires directly embedded in plaster.



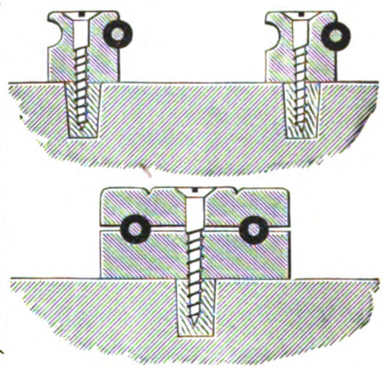
3. Wires secured on surface by wood cleats.



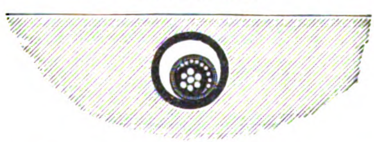
4. Wires secured on surface within wood casings.



5. Wires secured on surface by means of porcelain knobs or cleats.



6. "Conduit" wiring beneath surface with twin (concentric) conductors.



7. "Armoured conduit" wiring beneath surface with twin (parallel) conductors.

Mr. Bathurst.

APPENDIX B.

Table showing the "Insulation Test" required in various Countries.

(Compiled from Rules of Supply Companies and Fire Offices.)

Voltage of circuits not to exceed 250 volts.

Size of Installation, wired complete.	Country.			
	England (London).	America (New York).	Germany (Cologne).	Austria (Vienna).
Amperes.	Ohms.	Ohms.	Ohms.	Ohms.
10	500,000	2,000,000	100,000	50,000
25	250,000	1,800,000	40,000	20,000
50	125,000	400,000	20,000	10,000
100	—	150,000	10,000	5,000
200	—	80,000	—	—
250	25,000	—	4,000	2,000
400	—	40,000	—	—
500	12,500	—	2,000	1,000
800	—	11,000	—	—
1,000	—	—	1,000	500
1,600	—	5,500	—	—

APPENDIX C.

Resolutions adopted at the Meeting of the Underwriters' International Electric Association, Chicago, August 17th, 1893.

Whereas, under existing practices, but extremely few of the buildings in the United States which are wired are inspected before being closed in by lath and plaster; and

Whereas, an inspection after such closing in is incompetent and worthless, for the reason no means are known to the electrical

profession by which the fire hazard of concealed or inaccessible circuits may be determined, and that a visual examination of all circuits must be had before immunity from fire hazard can be assured; and Mr. Bathurst.

WHEREAS, by the reason of non-inspection, the work of incompetent and irresponsible electricians remains for ever permanently concealed and uninspected, thereby affording ample opportunity for evading the requirements for safety, for practising gross impositions upon fire underwriters and the public, and imperilling millions in property valuations; and

WHEREAS, only through the use of insulating wireways is it possible to render concealed wiring permanently accessible; and

WHEREAS, such permanent accessibility renders it possible to thoroughly inspect at any time every installation so wired, and ensures trustworthy means for the detection of every piece of defective wiring of whatever nature: therefore, be it

Resolved, that the use of such insulating wireways for rendering concealed wiring permanently accessible be, and hereby is, most heartily endorsed and recommended; and be it further

Resolved, that such methods of accessible concealed construction be, and hereby are, advised for general use.

Resolved, that the secretary be requested to communicate with the American Institute of Architects, asking co-operation of their Association in an endeavour to have architects, when drawing plans for specifications, make provisions for the channelling and pocketing of buildings for electric light or power wires.

Resolved, that they also be requested in specifications for electric gas lighting to require a two-wire circuit, whether the building is to be wired for electric lighting or not, and that no part of the gas fixtures or gas piping be allowed to be used for the gas lighting circuit.

The PRESIDENT: Gentlemen,—In rising to propose a vote of thanks to the author of this paper, I venture to suggest that it would be very convenient if we take the next paper before we enter upon the discussion of this one, so that we may discuss both papers at once. The President.

The vote was unanimously carried.

The following paper was then read :—

CONCENTRIC WIRING.

By Sam. Mavor, Member.

Mr. Mavor Considering the importance of the subject of electric light wiring, it does not seem to have had an adequate share of the attention of this Institution. This is the more unfortunate as the tendency now is to allow the business of wiring to drift out of the hands of experienced electrical engineers into the hands of the legion of plumbers, gasfitters, and others who, often with small claim to the title, write "Electrical Engineer" upon their signboards. It is inevitable that wiring shall fall more and more into the hands of tradesmen of this class. They will follow more or less closely the methods of their predecessors, and defects in existing practice will thus be widely propagated. This is a question which seriously affects the whole industry, and hence the importance of this Institution setting an example by aiming at and endeavouring to secure a high standard of excellence in wiring work. It is a characteristic of the business of electric light wiring that it is peculiarly liable to the risks attending bad workmanship. Internal wiring—that is, the laying of insulated conductors in grooved wood casings, the jointing of these conductors, and the fixing of switches, fuses, &c.—is a comparatively simple matter. Apart from the distribution of lights and arranging the circuits and their routes, it is not work which must necessarily be done by a highly trained and skilled tradesman. For such work as the mason's, the bricklayer's, the joiner's, cabinetmaker's, &c., a man requires the training of a long apprenticeship, which entitles him to the wages of a skilled craftsman; but the mere laying of wires already manufactured in grooved wood casings erected by a joiner, and fixing ready-made switches on bases also prepared by the joiner, do not require the serving of a long and laborious apprenticeship. On the contrary, any man who can use his hands may, with very short experience, learn to joint wires and fix switches, and then call himself a wireman. The inevitable result is that such labour can, and always will, be cheaply purchased. Much wiring work is really done by irresponsible amateurs of this

kind, who are ignorant of the consequences which might result from their carelessness or lack of experience. Mr. Mavor.

A further reason why electric light wiring is often not carried out by first-class labour lies in the intermittent nature of the employment. There are certain seasons of the year when the wiring contractor has little of such work, and must then discharge men. Indeed, wiremen are many times taken on at the beginning of a job and paid off at the end of it. Workmen of skill and intelligence soon find this out, and they seek employment in branches of the business where their qualifications find a better and more steady market. We all know how demoralising it is for men to be idle for months at a time, and how apt they are to drift into lazy and careless habits. The accompanying diagram shows the number of 8-candle-power lamps connected to the Glasgow Corporation mains during each month from January, 1893, till the present time. The curve illustrates the fluctuations in the demand for wiring, and you will see that the wireman's load-factor is decidedly unsatisfactory.

You cannot expect a man to have a thorough interest and sense of responsibility in his work who knows that he will be paid off at the completion of the job. As wiring jobs often last only a few weeks, the poor wireman may have been in the employment of half a dozen masters in the course of a season, with intervals of enforced idleness. In the case of joiners and plumbers and kindred trades, when outside work is slack, the men may be employed in the shop at preparing material or in manufacturing; but not so with the wireman. The material is all already prepared for him by skilled hands,—his business is only to erect it. The larger firms who have a variety of electrical work can find employment for the best of their wiremen in other branches when internal wiring is dull. But they, too, when business is brisk, have to fall back upon the shiftless creatures who often present themselves as "wiremen." The proportion of "improvers" and apprentices employed at wiring is very much larger than in any similar trade.

The business—if it may be called a business—of electric light wiring has thus several features peculiar to it making for

Mr. Mavor. unreliable workmanship, which, in the absence of competent supervision and careful inspection, lead to defective work and resulting fire risk.

The plumber's and the gasfitter's work are to a great extent self-testing. If a water pipe leaks we see the water, and if a gas pipe leaks we usually smell the gas; but we have no sense—no physiological sense—to enable us to find hidden defects in our electric wiring. A building may be wired in the most dangerously slipshod style, yet the lamps may for years burn none the less brightly, until the latent faults have opportunity to discover themselves.

Insulation tests after the completion of the work, if in dry and well-seasoned buildings, are of little value. The insulation tests of the wiring of an old building might show results three or four times higher than the most stringent rules require, and yet there might be a score of dangerous defects only awaiting opportunity to develop. No tests can be applied for the detection of these defects after the work is completed and covered in. On the other hand, in a new and damp building the insulation might be far below the requirements of the rules, and yet be absolutely safe. The important thing is that the work shall be carried out in such a style that the insulation attained shall be permanent, and tend to increase, rather than decrease, with time. Wiring has to be carried out in situations so different, and must comply with conditions so varied, that it is impossible to formulate rules which can in all cases be adhered to. Modifications must be allowed to meet special requirements, and the extent of these must be left to a great extent to the discretion of the contractor. That such discretion ranges over very wide latitudes is indicated by the large discrepancies between the tenders of different contractors for the same wiring work. When the prices of experienced contractors estimating for wiring work to a carefully drawn up specification vary as we know they do, it is not strange that inexperienced contractors tendering in the absence of a specification, and without the prospect of any supervision, should quote prices which would not purchase suitable materials alone. These are matters of primary importance to the fire insurance

companies, and they are much too lax in looking to their interests in this regard. Bad work is being carried out every day, and the insurance companies are complacently taking the risk. Several offices have issued wiring rules, and it is a very good thing for them to have rules; but what is the use of rules if they are not enforced? At present they are really only recommendations, and anyone who is so inclined may evade or ignore them. It is common knowledge that this is constantly done. Mr. Mavor.

The present procedure of the insurance companies is most ingenuous. They send a schedule to the contractor who is carrying out the work—or, rather, who has done the work, for it generally arrives after the job is completed—and they then request the contractor to oblige them by giving replies to a list of questions. They receive the document filled up by the contractor, who has no responsibility beyond a moral one, and file it away, and presumably are satisfied that the thing is all right. They thereby exhibit a trust in human nature very flattering to the wiring contractor, but of the wisdom of which some of us may have our doubts. Until the insurance companies have organised a thorough system of inspection they will not have taken the steps necessary for their own protection. It seems to me that in every considerable town there ought to be a permanent official (in the pay of the fire offices collectively) whose duty it would be to inspect all electric light wiring while in course of erection, and to ruthlessly condemn defective workmanship or material. If such a staff of well-chosen, reliable, and sufficiently well paid inspectors were scattered over the country, they would soon put a stop to jerry work. The moral influence of their presence in the town would be a wholesome check upon the cheap jack. The cost when spread over all the companies would be trifling, and would be amply repaid by the reduction of fire risk. The insurance companies already have one man in their collective employment in each of several of the larger cities. This man is an *attaché* of the Salvage Corps, and his duties are confined to the warehouse district. His functions are to familiarise himself with the positions of main switches, fuses, and cables, &c., in the warehouses, with a view to his being of service to the Fire Brigade in case of emergency.

Mr. Mavor. Before leaving this subject it may be noted that Lloyd's Register of Shipping some years ago issued a set of recommendations for the guidance of shipbuilders and owners in fitting electric light on their vessels; but, as they took no steps to ensure that effect was given to their suggestions, these remained a dead letter. The large number of electric fires on board ship during recent years, chiefly due to inferior work resulting from the practice of accepting the lowest tender, has impressed upon Lloyd's the imperative necessity for supervision. They have, therefore, issued a new set of rules, and they are now arranging for such a system of inspection as will ensure these rules being followed. The fire insurance companies would do well to profit by the dearly bought experience of Lloyd's. If they do not, they will assuredly have to learn the lesson at their own expense. Although the insurance companies are primarily interested in this question of fire risk, the matter has a direct and important bearing upon the electric lighting industry. We must all recognise the desirableness of ensuring that wiring shall be carried out in a style which shall be fire-proof, electrically and mechanically good and durable, and shall not be offensive to the eye.

There is a steadily growing dissatisfaction with the generally adopted practice of enclosing the conductors in wood casings, and a steadily growing conviction that something better might be done. In most factories and warehouses it is possible to have the casings laid upon the surface, but in dwelling-houses or in good hotels or clubs it should not be tolerated. Everyone is familiar with the unsightly wood casings and clumsy crockery ware which disfigure so many of the best hotels and other buildings in this country. How often we hear that electric light is not adopted because of the dread of the wood casings, and the cutting about and disturbance which their erection involves! Those casings are a standing reproach to the people who erect them. One has only to see the interior of a good building which a contractor has dared to wire on the prescribed lines with wood casing to realise how utterly unsatisfactory is the system. Those hideous casings are entirely out of harmony with their surroundings, and offend the eye at every turn. They blunder across ceilings and cornices,

intrude upon decorative panels, and push their ruthless way through frieze and dado; while out of sight beneath the floors the joists are cut away to clear the casings until the margin of strength is often perilously low. The truth of the matter—and we all know it—is that we cannot carry out the wiring of, for example, a private residence in compliance with the recognised rules for double wiring and wood casing without excessive cutting of floors and plaster work, and disfigurement of walls and ceilings. At every turn we have to compromise between the decorations of the house and the fire insurance rules, and the concessions must needs be generally in favour of the decorations. In short, the modifications of the rules require to be so many and important that this system of wood casing is one which cannot be consistently and thoroughly carried out. Mr. Mavor.

When the usual double wiring is carried out in situations where moisture exists charged with acid from cemented or plastered walls of interiors, or in such buildings as paper mills, wet spinning mills, dye houses, breweries, distilleries, chemical works, and the like, the destruction of the insulation is only a matter of time, and the casing, by harbouring the moisture around the conductors, hastens the process. After the insulation has broken down, electrolysis, assisted by the acidulated moisture, takes place, and deposits away the copper of one of the conductors, with the consequent risk of heating and ultimately of sparking. The presence of moisture being a necessary condition of the development of such faults, the dampness of the surroundings of the conductors greatly reduces the danger of fire, but that the risk is not absent is proved by the charred and burned casing or woodwork found at such places. It must be remembered, however, that heating due to diminished section of the conductor may occur at a time when the moisture which caused the fault has disappeared, and this is a danger which no amount of fuses will modify. How can you ensure that your wood-cased conductors shall never be subjected to such conditions? And how many of your average wireman's joints would stand 24 hours' immersion in water or soaking in moisture? The miserably futile expedient of attempting to waterproof the casings with putty and

Mr. Mavor. varnish is sometimes resorted to; such makeshifts are too childish. Lead-covered conductors are ocasionally used where much moisture has to be encountered. But these for double wiring are a mistake, unless the positive and negative wires are both enclosed in the same lead sheathing. The objection to separate lead-covered conductors is that, if there should be an earth or leak on the negative wire, and the lead and insulation of the positive wire becomes punctured at a damp situation, the lead becomes charged positively, and will be deposited away over its whole length where moisture is present to assist electrolysis. This remark has no application to lead-covered conductors on board ship, where the lead sheathing is throughout in intimate contact with the hull of the ship, nor to lead-covered concentric conductors. This liability to injury from moisture is the fatal weakness of the ordinary wood-cased system. It is of much greater consequence to obtain a moderately high insulation which may be relied upon to be durable, than to have—as can easily be had in a dry building—an insulation resistance of many megohms which will disappear on the approach of the charwoman and her wash-bucket, or is at the mercy of the first loose slate or leaky water pipe. What a common experience it is to discover wiring faults in the fungus-grown casings in damp and mouldy basements, or upon the " sweating " surfaces of cemented walls or ceilings! A fault which developed a few weeks ago in the writer's office is a good illustration of the weakness of the present double-wiring practice. The walls are covered with a light green paper, which on one side of the room is pasted to a wooden partition, and through this partition and paper the conductors are led to a switch. Behind the base of this switch sparking was observed. On examination, it was found that in our humid climate the paper had absorbed moisture from the atmosphere, and this moisture, charged with an acid held in the paper, had attacked and destroyed, first the insulation of the conductors, and then the copper, until of the latter only a green sulphate remained, and the circuit was interrupted. As only one lamp was in circuit the spark was small and the heating effect slight, but under other conditions the results might have been serious. This is an

interesting example of a fault developing in an apparently dry and well aired and warmed room, and under the eyes of one whose business it is to avoid such faults, and to whom a spark was the first intimation of anything being wrong. Mr. Mavor.

After wood casing, with its perviousness to moisture and other attendant disadvantages, the biggest blot on existing practice is the indiscriminate use of fuses. Some insurance people, and wiring contractors too, imagine that the risk of fire varies inversely as the number of fuses. The contrary is more near the truth. Fuses are not only a nuisance, they are apt to be a positive source of danger. The smaller number of fuses used the better; but they must be absolutely reliable, well mounted, and well placed. Most of the leading wiring contractors have adopted the distributing box plan of wiring, in which the branch fuses are grouped at the distributing centres. This system should now be compulsory. The use of isolated fuses should be strictly prohibited. The only inducement to use them is a trifling saving in the cost of conductors. The number of fuses should be kept down by keeping up the section of conductors. The larger expenditure on branch conductors entailed is amply repaid by the simplicity and uniformity secured. The dangers of diminishing the section of conductors and scattering fuses through a building are that the crockery fuse bases and cases are liable to breakage; the terminal screws are liable to become loose and so cause heating; and last, but most important, the danger of a conductor of too large area being used by an inexperienced or careless person instead of a *bonâ fide* fuse, in which case the "fuse" is a delusion. This last danger is so real and so imminent that the insurance companies ought long ago to have prohibited the use of such fuses. The writer was recently in competition with a firm of contractors of large experience who do a great amount of wiring, and were in this case estimating for wiring about 200 lamps of 16 candle-power each. Samples of the conductors, &c., quoted for were invited along with the estimates, and our double-wiring friends submitted a sample of each of the *nine* sizes of insulated cables and wires, with their corresponding wood casings, and of each of the several sizes of fuses which they proposed to

Mr. Mavor. use. Nine different sizes of cables and wires were certainly not essential to the double wiring of this building, but they were part of the method of a much-experienced contractor, and his method is common to many others.

Switches have been greatly improved during the last few years, both in mechanical design and in external form. The clumsy switches, with the ugliness of their porcelain covers embellished by gilt lines or floral decorations, have now nearly disappeared. They are superseded by switches of neat appearance, with suitable metal covers. The working parts of the switches are of a much more mechanical type than formerly, but there still remains ample room for improvement. There are far too many parts, too many screws, and too many contacts. The practice of dispensing with binding screws and soldering the wires directly to the contact plates should be highly commended. How often one sees the attempt made to secure the ends of conductors—frequently of considerable size—under paltry little cheese- or round-headed screws and washers absurdly too light and inadequate to the purpose!

The ceiling rose is also a weak spot in double wiring. There are far too many contacts in it. To begin with, there is the usual crockery base, and upon this base are three little brass plates, with twice that number of brass screws. Each of these six little screws binds to a contact the end of a conductor or fuse, and every screw of them is liable to become loose, and every contact to become oxidised. Where vibration or moisture are present the development of trouble is only a question of time. Do those who so freely erect these little collections of screws and contacts ever consider the harvest of petty troubles and annoyances which will be reaped by-and-by?

We must now pursue our painful path of adverse criticism to the lamp-holder. The double-contact lamp-holder, especially when attached to flexible pendants, is the frequent seat of short-circuit and minor troubles. In this little piece of apparatus there are two contact plates, carrying the buffer springs and the inevitable binding screws. Each of these contact plates must be insulated from the other, and both from the surrounding metal

tube. When this is done, small space remains for further intrusion. But the flexible conductors are still to be brought in and pinched under the binding screws. Stray strands from these flexibles, where they are not most carefully soldered, are frequently responsible for short-circuits and the melting of many fuses. The risk would be reduced if here also binding screws were dispensed with and the wires brought straight through the insulating disc into holes in the plates and soldered therein. Mr. Mavor.

It is with a sense of relief that one turns from double wiring and all its attendant ills to the simple mechanical and straightforward methods of concentric wiring with an uninsulated outer conductor. So far as the writer is aware, no system of concentric wiring in which both the inner and outer conductors are insulated has ever been used for indoor work. It appears probable that the difficulties in the way of designing junctions, switches, and other accessories which shall meet the electrical and mechanical requirements within moderate dimensions and at reasonable cost, will prevent the introduction of such a system. Two- and three-wire concentric cables with each conductor insulated are largely used as mains in connection with two- and three-wire systems, but in such cases there is no difficulty in providing junction boxes of adequate size. The only system of concentric wiring which has been adopted for internal use has an uninsulated outer conductor, and it is this system that is known as "Concentric Wiring." It is important that it be clearly understood that concentric wiring is not "single wiring," but is something essentially different. This distinction between concentric wiring and single wiring is necessary, because the two have been frequently confused. No method of wiring merits the title "concentric" which is not consistently concentric throughout. In a concentric system the central conductor must be everywhere surrounded over its insulation by a metallic sheathing of conductivity equal to or greater than the core. The switches and fuses should be in the central wire and enclosed in metal cases. These cases or boxes should be electrically and mechanically jointed to the outer conductor, and so form part of the continuous metallic envelope in which the central conductor is enclosed.

Mr. Mavor. It must be pointed out here that, under the existing Board of Trade regulations referring to the insulation of electric light supply mains, concentric wiring is not admissible where connection has to be made to a source of public supply in this country. This is a matter to which we shall recur, and shall meanwhile consider the question of concentric wiring with regard to installations supplied with current from independent plants. The adoption of concentric wiring is the royal road out of the difficulties and dangers which beset double wiring. It seems to be difficult for those who have been accustomed to devote so much pains to attain good insulation of both conductors to realise that there is no necessity for doing so. The adoption of concentric wiring dispenses with this necessity, and abolishes risks which make a high standard of insulation of both conductors in a two-wire system so desirable. Much misunderstanding and prejudice, due to lack of information, exist with regard to concentric wiring; but there is complete absence of valid objections to its use, and prejudice against it is soon dissolved by an acquaintance with the system.

Some of the fire insurance companies were at one time rather suspicious of concentric wiring, probably because they feared its adoption would lead to the abolishing of their cherished rules regarding the width of fillets in wood casing, &c. The insurance companies are, however, now unanimous in their favourable verdict on the subject, and have recognised the advantages of concentric wiring from their point of view. More extended experience and appreciation of its fire-proof qualities must confirm the preference many of them now have for concentric wiring.

It has been stated that, owing to one conductor being already earthed, the liability to complete breakdown of the insulation is doubled. This might be true of double wiring, but is certainly not true of concentric. The outer conductor being already at earth potential, there cannot be any risk whatever of shock or spark being experienced from it. There is thus only one vulnerable conductor, and this, with its insulation being encased throughout its entire length in the metallic sheathing formed by the outer conductor, is well protected against injury. The consequences of

injury to concentric and to double wiring are essentially different. Mr. Mavor. In the case of the concentric conductor, if it should be crushed or a nail be driven into it, the instantaneous result is a dead short-circuit and the melting of the fuse. If the insulation should be punctured and moisture gain access, the distance across the insulation between the conductors is so small that the fault immediately develops into a short-circuit, and the spark passes from the centre conductor to the inner surface of the metallic sheathing, and being entirely within the sheathing it cannot communicate fire to its surroundings. Concentric conductors are thus self-testing. Faults cannot endure. They cut themselves out automatically. Or, rather, this is what they would do if faults were experienced, but we never have faults in lead-covered concentric conductors. How different is the case of ordinary double-wire conductors laid in the separate grooves of a wood casing! The casing which is provided for mechanical protection may become a positive source of danger, and may be ignited by leakage across the fillet—a danger which the fuse is powerless to prevent. In concentric wiring there is only one cable to handle instead of two, and this carrying its own protection the necessity for wood casing is abolished.

Perhaps the most conspicuous advantage of concentric wiring is that it may be so easily and so effectually made waterproof. The cables may be enclosed in lead sheathing laid under or over the outer conductor, and hermetically sealed to the various apparatus into which the cables are led. This must appeal to all those who have had a varied experience of wiring. If moisture—the arch enemy of good insulation—can be permanently excluded, it means the permanent maintenance of a high standard of insulation, and consequent freedom from trouble.

The next important advantage to be claimed is the large reduction in the number of fuses. The drastic method of abolishing fuses is to maintain the section of the conductors. This can readily be done in concentric wiring to an extent impracticable in double wiring. Conductors of much larger section may be conveniently and safely led into C.C. lamp-holders, ceiling roses, and wall sockets, than is possible in double wiring.

Mr. Mavor. And thus, by maintaining the section of conductors right into the lamp-holders, many of the objectionable fuses are abolished. The fuses when reduced in number are more easily centralised and arranged in groups of standard size. It may be objected that if the number of fuses is diminished a larger number of lamps are affected in the event of a fuse being melted by a short-circuit. This is a very natural objection for one experienced in double wiring to offer. But it has no application to concentric wiring. We never have short-circuits. It is in double-contact lamp-holders, and double-wire flexibles, ceiling roses, and the like, that short-circuits occur. Compare a central-contact and a double-contact lamp-holder. A 7/16 concentric cable may be led into the former more easily, and with greater safety, than two single No. 16 L.S.G wires or their equivalent can be led into the latter. It is not necessary to lead 7/16 cables into concentric lamp-holders, but the possibility suggests the large margin of safety in favour of the C.C. holders. The difficulty of avoiding mishaps in double-wire lamp-holders will be emphasised by the introduction of 200-volt lamps. While on the subject of lamp-holders, it may be noted that, there being only one buffer spring in the C.C. lamp-holder, and that exactly in the centre, there is less risk of twisting lamps out of their collars while inserting or removing them. Concentric lamp-holders, ceiling roses, and such fittings, having only one—and that a central—contact to be insulated, may be made of better mechanical design than where within the same space two contacts have to be insulated, each from the other, and both from the containing case. The benefit of the great reduction in the number of parts can hardly be exaggerated, the liability to derangement is so very greatly reduced.

In regard to fittings, the comparison is again in favour of concentric wiring. The flexible conductors of which the pendants are made are of such large section that they are mechanically strong. The pendant terminates at either end in a screwed brass nipple with insulating plug and central contact—an arrangement which is substantial without being clumsy. No loose strands are possible. The top nipple of the pendant is screwed into the junction or fitting base, and the nipple at the lower end is

screwed into the lamp-holder. The brackets are similarly wired Mr. Mavor. to screwed nipples with central contacts. The erection of fittings is therefore an extremely simple operation, and the labour so employed is reduced to a minimum.

The method of concentric wiring illustrated by the patterns before you is based upon a full recognition of the fact that electric light wiring, in order to be permanently durable and reliable, must be impervious to moisture. So far as the writer is aware, this is the only method of wiring which provides a continuously waterproof metallic sheathing over the insulation from the main switch-board to the lamp-holders. The main switch-board has the usual single-pole switch and fuse arrangement, and the board is surrounded by the negative omnibus bar, which receives in sockets attached to it the outer conductors and sheathings of the concentric cables. The main cables—which are lead-sheathed throughout, and are generally armoured with galvanised iron wires laid over the lead on a cushion of jute—are carried without break or joint direct to their respective distributing boxes, where the central conductors are soldered to the omnibus bars, and the outer conductors terminate in gun-metal sockets secured to the boxes. The distributing boxes are of cast iron or cast brass, enamelled white inside, and fitted with fuses, or switches and fuses, as required. These boxes have close backs and hinged fronts closing upon an india-rubber ring, thus rendering them proof against dust or moisture. The concentric branch cables which radiate from these boxes are under all ordinary circumstances of uniform section, namely, 7/21½—equal in area to 0·005 square inch. The outer conductor of the cable has the same section of copper as the core, and the whole is enclosed in a solid drawn tube of lead. Wherever a joint is to be made, or the cable led into a switch or fitting base, the centre wire is soldered to its contacts, and the outer conductor and its lead sheathing are received and terminate in a jointing pocket cast upon the switch-box or junction. The central wire and its insulation are thus enclosed throughout their length in a hermetically sealed metallic sheathing. The section of the branch conductor 0·005 square inch is carried into every switch and into every

Mr Mavor. lamp-holder. no reduction being made. There is no necessity for any fuses other than those in the cast-iron fuse-boxes, and these are all uniform and interchangeable. It is very rarely that occasion requires any departure from this plan. Many large installations have been carried out on the lines indicated, some of these amounting to several thousands of lamps, where only two sizes of fuses—main and branch, respectively uniform and interchangeable—have been used. The simplicity and reliability of such a system must be too obvious for emphasis.

The branch cables are led through buildings like flexible gas pipes, but they are not liable to damage as composition gas pipes are. The conductors and the insulation within the lead sheathing serve as a backing for the lead, so that it is not at all easily damaged. It has been the custom of the writer's firm for years to have their concentric conductors and joints embedded in the plaster of new buildings, and they have never known a single instance of a fault developing in conductors so treated. It is sometimes objected that where the conductors are so buried they are not accessible. They certainly are not. Why should they be? It is essential that wood casings carrying ordinary double wiring should be accessible; further, it is desirable that they should be visible wherever possible. But the conditions with regard to lead-covered concentric conductors are entirely different. Given a conductor and joint of imperishable materials, and impervious to moisture and the acids found in plaster and cement, and no reason remains for laying conductors on the surface, or of providing access to them where enclosed in plaster. This demand for accessibility, and the objection to cover conductors with plaster, is a relic of the times when india-rubber-covered conductors laid in wood casing were embedded in walls. The bitter experience which rapidly follows this practice results in the prejudice against covering any conductors with plaster. If anyone wishes to have his conductors laid on the surface of plastered walls, by all means let him do so, but there is no necessity either to lay them on the surface or to provide access to them where lead-covered concentric conductors are used. Access to the distributing boxes only is necessary.

Another bogey is the chance nail driven into the plaster. Mr. Mavor. Consider how largely the practice of embedding composition gas pipes in plaster is in vogue, and how immeasurably greater is the danger resulting from the piercing of such a pipe than from piercing the insulation of a conductor. Yet how seldom one hears any objection to the practice! If a nail is driven into a concentric conductor the immediate result is an emphatic short-circuit, and the fuse melts; there is not the slightest danger. Two cases only of nails piercing embedded conductors have come under the writer's notice. Both of these were in the same building—a block wired for about 500 lights, where all the conductors were embedded in plaster. The faults—one behind a picture moulding, and the other behind a dado—were readily localised, and repaired before the work was completed.

The insulation resistance of the wiring in a new building is not a matter which causes any anxiety. The lead sheathings and brass junctions and switch-boxes make one independent in this regard. In occupied buildings the lead-covered branch conductors may be led about under floors and behind plaster and linings with the greatest freedom and complete immunity from danger. The amount of lifting of flooring and disturbance to woodwork and plaster is only a fraction of that required for the carrying out of wood-cased double wiring.

The junctions employed, specimens of which are upon the table, may be manipulated by any intelligent man who can use a soldering bolt. No careful lapping of insulating material around the joint is required. The insulation is air space, and there is none better. The branch conductors being of uniform size—so are the junctions—and men soon become expert in jointing. The operation is so very simple and easily performed that it may safely be done by unskilled labour. This feature specially adapts the system for export. As an illustration, it may be stated that a squad of Hindoos, chiefly tinsmiths, under the guidance of one European, carried out in the most satisfactory style the wiring for 3,000 lamps in ten weeks.

In mills or factories floored with wood the conductors are cleated to the woodwork with brass cleats; and where iron beams and

Mr Mavor. concrete floors are met, the conductors, with junctions (tapped to receive the pendants) attached at the proper intervals, are secured by brass or copper cleats to stranded steel suspending wires strained between eye-bolts attached to the walls. The laborious plugging of the concrete ceilings, with the resulting dust and disturbance to the work of the factory and expense to the wiring contractor, is entirely abolished. In single-story weaving sheds, iron works, or other buildings with iron roofs divided into bays, the same method of suspending the conductors between the beams or tie-rods is adopted.

In support of the claim that this method of wiring is reliably waterproof, the case of Messrs. Nobel's Explosives Company's West Quarter Factory may be cited. The buildings to be lighted are isolated and scattered over a large area in order to reduce the risk and minimise the effect of explosions. The regulations issued by the Home Office regarding electric lighting were so very stringent that it was impracticable to comply with them except by the use of concentric wiring. With concentric wiring everything was made easy. Its water-proof and fire-proof properties overcame all the difficulties. Every joint and every yard of the conductors is outside and exposed to the weather, cleated for the most part to the gangways. The switches, of cast brass, and distributing boxes, of cast iron, are also all outside, protected only by water-sheds. The work of wiring was completed more than three years ago, and, notwithstanding three years' exposure to the elements in this climate, the insulation resistance is as high as ever. Monthly tests of the insulation are required by the Home Office, and on the 10th of last month the insulation resistance was two and a half times higher than the most stringent of the fire office rules require for *indoor* wiring. In the nitrating department of the same factory, which is a quarter of a mile distant from the other installation, the insulation is equally satisfactory, although the wiring is subjected to the profuse fumes of nitric acid. It is noteworthy that the insulation of the wiring at both these places is not india-rubber, but is fibrous. The exclusion of moisture is therefore solely due to the lead sheathing and waterproof nature of the joints.

The more difficult the wiring of a building appears to be, the more favourable is the comparison to concentric wiring. The iron roofs, concrete floors, moisture, and other ills that vex the spirit of the double wirer, have no terrors for his concentric friend. For wiring in any situation, from collieries to boudoirs, where difficulties of erection have to be faced, or in situations where the conductors, &c., after erection must endure excessive heat or cold, moisture, acid vapour, or inflammable dust, concentric wiring meets the case. Conviction of its immense superiority is immediately impressed by inspection of buildings so wired. The thing requires to be seen to be appreciated. Concentric wiring has this further advantage—that a very simple and easily followed set of rules can be framed which are applicable to wiring to be carried out under widely differing conditions. Mr. Mavor.

Contractors who are unacquainted with concentric wiring are often ready, when they know it is in competition with their double-wire tenders, to pour into the ears of their prospective client a dismal tale of the woes which would follow the adoption of any system which would not embrace their precious wood casing and crockery ware.

If those contractors were informed on the subject, they would learn how great is their error. Take a contractor who has had experience of concentric wiring into any building whatever, and he will show how to make a better, more mechanical, reliable, and durable job, and generally at less cost, than is possible with double wiring.

Those who have used only double wiring, and know no other, are reluctant, and with reason, to depart from practice in which their patterns are systematised and standardised, and in which their staff are trained. But when the change proposed is all in the direction of simplicity and reduction of labour the chief objections to its adoption are removed.

That fires or minor troubles are not more frequent in badly wired buildings is a fine testimonial to the safety of electric light wiring, but is no excuse for the continuance of slipshod work or adherence to existing methods where improvements are possible.

For mining work concentric wiring is specially adapted. The

Mr. Mavor. mechanical nature of the fittings, and the ease with which they and the conductors may be made water-proof, are important features. A concentric cable for mining purposes is more safe than two separate cables. A fall of material from the roof may rupture one of two conductors, and if current is passing the inevitable result is a spark at the point of parting. In fiery mines this might have serious consequences. With a concentric conductor, however, the fall would crush the outer conductor in upon the core, and so cause a dead short-circuit and melt the fuse before the cable parted. The spark would thus take place at the fuse at the pit bank. In the only case of such accident within the writer's experience the fuse did promptly melt. Several devices have been proposed for the purpose of preventing a spark at the point of rupture of cables used in pit work. There is room for doubt as to the likelihood of these devices performing their functions in case of need. There can be no question that the concentric cable is much more simple, and it is probably more reliable than any of them. Further, none of these arrangements afford to the miner the immunity from personal danger from shock which the concentric cable does. An E.M.F. of 500 volts is frequently used for such work, and it is usually considered that such a pressure, although sufficient to give a disagreeable shock, is not dangerous to life. That this feeling of security is not well founded is unfortunately proved by recent fatal accidents. For power transmission in mines an ideally safe system is furnished by concentric cables with earthed sheathing. and switches, fuses, and other appliances enclosed in cast-iron cases, also earthed, and, if need be, enclosed-type motors with their casings earthed.

With regard to the wiring of ships, it is well known that an essential condition of durable wiring is that it shall be water-proof throughout. The British Admiralty for years tried all that the best materials of the usual double wiring sort, and most careful workmanship, could do to secure the desired result. But the incessantly recurring faults and derangements drove them six years ago to abandon wood casing and the ordinary rubber insulated conductors, and no longer to attempt the impossible. At the time indicated they discarded wood casing and adopted in

lead sheathing of the conductors the most effectual means of rendering them permanently water-proof. It was not, however, till four years later that they fully faced the question of making the wiring systematically water-proof throughout. Cast-brass distributing fuse-boxes, and cast-brass switch-boxes, all of water-proof patterns, were then adopted, and means taken to make watertight joints between these boxes and the lead conductors. The present patterns of distributing boxes, &c., although they serve their purpose, are still capable of improvement, and their awkward styles admirably illustrate the difficulty of designing water-proof fittings for use in a double-wire system, where the two sides of the circuit must be insulated from each other, and both from metal of the containing boxes. Mr. Mavor.

The Admiralty still adhere to double wiring owing to fancied danger of compass disturbance from concentric wiring. In the early days of electric lighting some of Her Majesty's ships were single wired and had dynamos of unsuitable types injudiciously placed. The results were compass disturbance. (The risks of compass disturbance even from single wiring are as nothing when compared with those which may arise from the dynamos.) The navigating officers naturally became alarmed, and the alarm has not yet subsided. That well-arranged concentric wiring has no effect whatever upon the compasses of a ship has been proved by the most careful tests. The water-proof qualities of concentric wiring and the safety from fire which it ensures peculiarly adapt it for use on board ship.

The history of the development of electric light wiring in our Navy is highly interesting and instructive. The conditions to be endured by wiring on board ship are admittedly severe, but it is under these circumstances that the inherent defects in a system will be most rapidly disclosed. We may profit by the experience of the Admiralty, and accept their progress as an index to the direction in which wiring practice on shore should be improved. The same defects which led to the abandonment of double wiring in wood casing in the Navy exist in this system when applied to land work. The conditions on shore are less trying, and severe treatment less frequent, but

Mr. Mavor. the same inherent defects are there, and only await opportunity to develop.

And now with regard to possible electrolysis due to difference of potential along the outer conductor. In isolated installations where the distances are short the question of electrolysis may be dismissed as having no bearing. In cases where the distances are considerable the risk of electrolysis may be entirely avoided by adopting very simple precautions. The first of these—suggested to the writer by Mr. T. B. Murray—is to so proportion the conductivities of the inner and outer conductors that the fall in volts shall be nearly all in the inner conductor; the copper saved from the inner conductor being added to the outer in order to reduce to a minimum the difference of potential between its ends. Another method adopted by the writer is to provide between the outer conductor and its lead sheathing a very light insulation, and to earth the outer conductor only at the distant end. The layer of braiding laid for manufacturing reasons over the strands of the outer conductor before it is passed through the lead press is sufficient for the purpose, as the maximum difference of potential between the outer conductor and the lead is only a few volts.

The risk of electrolysis is greatly over-estimated. The prejudice against earthing the return conductor is largely due to the results of the early American practice of using an earth return for street railway circuits, instead of providing an adequate return conductor. The consequence of this habit was serious damage by electrolysis to gas and water mains. But what were the conditions? In many cases the current was put to earth at the distant points at 50 volts to 100 volts above the E.M.F. of the dynamo negative, and the large currents, amounting sometimes to thousands of amperes, which had to find their way to the earth plates at the power house carried destruction in their path. But disappearance of these troubles has followed efficient bonding of the rails and the provision of complete metallic currents of suitable conductivity—in other words, the substitution of a metallic *earthed* return for an *earth* return.

In dealing with distribution for electric lighting purposes we do not have to contend with such large differences of potential at different points of the system, and the difficulties are not at all comparable with those encountered in street car work. A three-wire system with the middle earthed, and concentric wiring on each side with the sheathings connected to the middle wire, is an ideally safe and reliable system of distribution. In a well-balanced three-wire system the fall of volts along the middle wire is negligible, and there is therefore no danger of electrolysis. No disturbance to telephones could take place. Stray currents passing between faults in the conductors would be impossible. The positive and negative conductors being each surrounded by an earthed sheathing, no current can escape from them. Leaks in concentric cables will not endure. The distance between the inner conductor and its sheathing being only the thickness of the insulation, a fault develops at once into a short-circuit which is internal to the sheathing of the cable, and the fuse at once comes into operation. But it must be repeated that faults are much less liable to occur in concentric cables than in double wiring. The importance and safety of the self-testing properties of concentric wiring must be obvious. Mr. Mavor.

As distributing networks are enlarged and extended and the number of consumers increases it becomes cumulatively more difficult to keep up the pretence of insulation. The present supposed insulation of the three wires is a sham, and the sooner we acknowledge it the better. Experience has proved that good insulation of all three conductors in an extensive network cannot be reliably maintained. Then why not face the difficulties, and end them by earthing the middle wire?

The initiation by Mr. Sidney Baynes of the use of 200-volt lamps and 400-volt distribution on the three-wire system has a significant bearing upon this question. That his policy from the supply engineer's point of view is a sound one is beyond doubt, and that it will be largely followed in the near future is certain. But what about the interior wiring? If the attempt is to be continued to maintain the insulation of all three wires,

Mr. Mavor. the possibility is introduced of having a difference of potential of 400 volts between one of the wires and earth. The danger from leakage across damp woodwork and the like is therefore much increased, and possible danger to life introduced.

Recent developments and experience demand the reconsideration of this question of earthing in its relation to electric light conductors. The Board of Trade have issued regulations referring to the use of uninsulated conductors for tramway and railway work. The time is ripe for the framing of regulations relating to an uninsulated middle wire in the three-wire system of distribution.

Mr. MAVOR, having concluded the reading of his paper, said: May I, as an old pupil of Messrs. Crompton & Co., take this opportunity of expressing our sorrow and sympathy with our President in the calamity which has fallen on his firm—the partial destruction by fire of their works at Chelmsford? On hearing of the disaster, my first thought—and I am sure it would be the first thought of many of us—was, that I knew no man better fitted to cope with such an emergency, and to meet his difficulties, than Mr. Crompton. The splendid energy and indomitable courage, and power of work, which are some of his leading characteristics, and which have been examples and an inspiration to so many younger men, will soon raise over the ashes of the ruins at Chelmsford new works to sustain and enhance the reputation of his firm. I am sure that I echo the sentiments of all present when I say that Mr. Crompton in this trying and troublous time has our sincerest sympathy.

The meeting signified their complete acquiescence in Mr. Mavor's remarks.

The President. The PRESIDENT: I am sure you will give your customary thanks to Mr. Mavor for his pre-eminently useful and powerfully written paper on one of the most interesting subjects we could have brought before us at the present time. When he asked my permission to speak a second time, I did not know what he was going to say. I was thinking of something entirely different, namely, as to the best method of discussing these papers. I

think, considering the lateness of the hour, it would be better to take the discussion as a whole at the Annual General Meeting on the 12th December; but, as there may possibly be some gentlemen here who have come from a distance, and who would not be able to attend on December 12th, I would call on them to speak, as briefly as circumstances will allow, this evening. The President.

As regards what Mr. Mavor said about myself and my company and our disaster, I have nothing to say, except that I fully appreciate his and your kind sympathy, and that I hope we shall, as he has said, get over our difficulties as quickly as possible, and make as little fuss about it as possible.

As no gentlemen appear desirous of opening the discussion this evening, I adjourn the meeting until December 12th.

The meeting then adjourned.

The Twenty-fourth Annual General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 12th, 1895—Mr. R. E. CROMPTON, President, in the Chair.

The minutes of the Ordinary General Meeting held on November 28th were read and approved.

The names of new candidates for election into the Institution were announced and ordered to be suspended.

Mr. R. S. Erskine, Mr. C. O. Grimshaw, Mr. James Hookey, and Captain A. W. Stiffe were appointed scrutineers of the ballot for the election of Council and Officers for the year 1896, and for the election of new members.

The SECRETARY read the Annual Report of the Council, as follows:—

REPORT OF THE COUNCIL TO THE ANNUAL GENERAL MEETING, 12TH DECEMBER, 1895.

Inclusive of the candidates to be balloted for this evening, the total number of additions to the register during the year will have been 216, comprising 12 Foreign Members, 9 Members, 108 Associates, and 87 Students; and 38 candidates have been approved for election next month.

One Foreign Member and 14 Associates have been transferred to the class of Members, and 71 Students to the class of Associates.

DEATHS AND RESIGNATIONS.

The losses which the Institution has sustained by death, although larger than those of last year, are, fortunately, below the average. They are as follows:—3 *Foreign Members*—J. Aparicio, Colonel Hoskjor, Franklin L. Pope; 6 *Members*—J. R. Edwards, J. H. Greener, G. E. Hartmans, John Muirhead, Major-General R. H. Stotherd, J. Tasker; 6 *Associates*—H. C. Hart

W. H. Masters, C H. Raper, L. Schaefer, J. M. Smith, W. W. Turner.

Mr. Aparicio, who was originally in the Spanish Government Telegraph service, and subsequently the representative of the Direct Spanish Telegraph Co., had been a Foreign Member 21 years, and during the whole of that period had kindly acted as our Local Honorary Secretary in Spain, the duties of which office he performed with great assiduity. Colonel Hoskjor, of the Danish Royal Engineers, had been connected with the Institution almost from its formation. Mr. F. L. Pope's sad death was specially alluded to at the last meeting.

Messrs. Edwards, Greener, and Muirhead, as well as that distinguished officer Major-General Stotherd, R.E., were among the original members of the Institution, and the latter was a member of its first Council.

Four Foreign Members, 9 Members, 21 Associates, and 11 Students have resigned during the year.

Papers.

Besides the interesting and highly practical Address of the President, the following papers, dealing with nearly every branch of electrical engineering, have been read during the year:—

Date.	Title.	Author.
Jan. 24.—	The Origin and Development of the Telephone Switch-Board	J. E. Kingsbury, Associate.
Feb. 14.—	Reversible Regenerative Armatures and Short-Air-Space Dynamos	W. B. Sayers, Associate.
,, 21.—	Propagation of Magnetisation in Iron ...	Dr. John Hopkinson, F.R.S., Past-President.
Mar. 14.—	The Electrolysis of Gold	N. S. Keith, Member.
,, 28.—	On the Employment of the Electric Light for Railway Purposes	W. Langdon, Member.
April 25.—	A Magnetic Tester for Measuring Hysteresis in Sheet Iron	Professor Ewing, F.R.S. Member.
May 9.—	On the Recent Development of the Single-Acting High-Speed Engine for Central Station Work	Mark Robinson, Member.
Nov. 28.—	The Electric Wiring Question	F. Bathurst, Associate.
,, 28.—	Concentric Wiring	Sam. Mavor, Member.

The discussions which have followed the reading of these papers, and the very large attendances at the meetings, afford ample proof of the interest which they have evoked.

Annual Premiums.

In respect of the papers read at the Ordinary General Meetings during the session 1894-95, the Council have made the following awards, viz.:—

The Institution Premium (value £10) to Mr. Mark Robinson, Member, for his paper, "On the Recent Development of the Single-"Acting High-Speed Engine for Central Station Work."

The Paris Electrical Exhibition Premium (value £5) to Professor J. A. Ewing, F.R.S., Member, for his paper on "A Magnetic "Tester for Measuring Hysteresis in Sheet Iron."

The Fahie Premium (value £5) to Mr J. E. Kingsbury, Associate, for his paper, "The Origin and Development of the Telephone "Switch-Board."

The Students' Premium (value £3 3s.) they have awarded to Mr. A. C. Eborall for his paper on "Single-Phase Alternate-Current "Motors."

Salomons Scholarship.

Partly owing to a balance of the Scholarship Fund remaining unappropriated last year, and partly owing to the generosity of Sir David Salomons in having this year added £500 to the capital of the Fund, the Council are enabled to grant two Scholarships of £45 each, and these they have had pleasure in awarding to the following gentlemen, viz.:—

Percy Rhodes Cobb, of King's College, London.

Joseph Ernest Petavel, of University College, London.

Fire Risk (Wiring) Rules.

The general rules for the prevention of fire risks from electric lighting, drawn up by the Council in 1883, and remodelled in 1888, having, by reason of the great advance made in electric lighting, become in some respects practically obsolete, a committee of the whole Council have been, and are still, engaged in their

complete revision, and the new rules will, it is hoped, be ready for issue early next year.

Board of Trade.

The recent meeting on the subject of the revised regulations under the Electric Lighting Acts, 1882 and 1888, so admirably presided over by Sir Courtenay Boyle, affords further proof of the desire of the Board of Trade to learn the views of the profession, and to meet those views so far as is consistent with their duty to the public. They have consulted the Council of the Institution on important matters on more than one occasion during the past year.

Annual Conversazione.

The attendance of members and their friends at the Conversazione held at the Royal Institute of Painters in Water Colours on July 3rd was, as usual, very large.

Annual Dinner.

The attendance at the seventh Annual Dinner of the Institution, which takes place to-morrow, promises to be larger than that of previous similar occasions, and the members will be honoured by the presence of H.R.H. the Duke of Cambridge.

Building Fund.

Out of the surplus receipts of last year the Council, as they anticipated, have been able to place the sum of £500 to the credit of the "Building Fund," which now stands at £3,500, and they hope to add a further £500 out of the current year's surplus.

Financial Position.

The financial position of the Institution is satisfactory, and the annual accounts, when made up to the end of the year, will in all probability show a surplus equal to that of last year.

The following investments have been made during the year, viz.:—£554 17s. 8d. on account of "Life Compositions," and £1,099 19s. 6d. on account of "General Investment Fund."

Conclusion.

The roll of the Members and Associates of the Institution is being continually augmented from the ranks of the large number of engineers who are now employed in the electricity supply works which are being started in almost every town throughout the United Kingdom. Up to the present time 126 of these works are in operation, employing between 150 and 200 engineers, of whom the large majority are either Members or Associates of this Institution.

The large number of electro-chemical and electro-metallurgical works which are being opened in those parts of the country where fuel is cheap, and power can consequently be supplied at small cost, are also employing a very large number of our members. An immense field will doubtless be afforded for the employment of electrical engineers by electric traction, which, it is true, is yet in its infancy in this country; but there are signs that during the next few years this Institution will have its ranks largely increased from this source, as in the United States of America, engineers engaged in electric railway work form a very large percentage of the members of the electrical profession.

The Council believe they are, therefore, not only justified in congratulating the members on the general position of the Institution, which so largely represents every branch of the profession, but that they are warranted in expressing the opinion that new and important opportunities are about to be afforded, both in the United Kingdom and in our Colonies, for the exercise of the skill and ingenuity of the electrical engineer.

THE LIBRARY.

REPORT OF THE COUNCIL.

I beg to report that the accessions to the Library during the year number 68; of these, 8 were purchased, the remainder having been kindly presented either by the authors or the publishers.

The specifications of all electrical patents continue to be

supplied to the Institution, by the kindness of H.M Commissioners of Patents.

The number of patents applied for this year, up to November 27th, was 22,442, of which 1,367, or 6·09 per cent., were electrical.*

The corresponding numbers last year were 22,842 and 1,203, or 5·26 per cent.

The periodicals and printed proceedings of other Societies received regularly are, with some few additions, the same as last year, as may be seen by the list appended hereto.

The number of visitors to the Library to the end of November has been 743, of whom 116 were non-members.†

The corresponding numbers last year were 712 and 120 respectively.

F. H. WEBB,
Secretary.

APPENDIX TO SECRETARY'S REPORT.

TRANSACTIONS, PROCEEDINGS, &c., RECEIVED BY THE INSTITUTION.

ENGLISH.

Asiatic Society of Bengal, Journal and Proceedings.
Greenwich Magnetical and Meteorological Observations.
Institute of Patent Agents, Transactions.
Institution of Civil Engineers, Proceedings.
Institution of Mechanical Engineers, Proceedings.
Iron and Steel Institute, Proceedings.
King's College Calendar.
Liverpool Engineering Society, Proceedings.
Physical Society, Proceedings.
Royal Dublin Society, Transactions and Proceedings.
Royal Engineers' Institute, Proceedings
Royal Institution, Proceedings.
Royal Meteorological Society, Proceedings.
Royal Society, Proceedings.
‡Royal Society, Philosophical Transactions.

* Up to December 31st the number applied for was 25,053, of which 1,509, or 6·02 per cent., were electrical.—Sec.

† Up to December 31st the numbers were 811 and 125 respectively.

‡ Presented by Professor D. E. Hughes, F.R.S (Past-President).

Royal United Service Institution, Proceedings.
Society of Arts, Journal.
Society of Chemical Industry, Journal.
Society of Engineers, Proceedings.
University College Calendar.

AMERICAN.

American Academy of Science and Arts, Proceedings.
American Institute of Electrical Engineers, Transactions.
Canadian Society of Civil Engineers, Transactions.
Franklin Institute, Journal.
John Hopkins University Circulars.
Library Bulletin of Cornell University.
Ordnance Department of the United States, Notes.
Technology Quarterly.

FRENCH.

Association des Ingénieurs Électriciens sortis de l'Institut Électro-Technique Montefiore, Bulletin.
Académie des Sciences, Comptes Rendus Hebdomadaires des Séances.
Société Belge d'Électriciens, Bulletin.
Société Française de Physique, Séances.
Société des Ingénieurs Civils, Mémoires.
Société Internationale des Électriciens, Bulletin.
Société Scientifique Industrielle de Marseille, Bulletin.

LIST OF PERIODICALS RECEIVED BY THE INSTITUTION.

ENGLISH.

Cassier's Magazine.
Electrical Engineer.
Electrical Plant.
Electrical Review
Electrician.
Electricity.
Engineer.
Engineering.
English Mechanic and World of Science.
Illustrated Official Journal, Patents.
Industries and Iron.
Lightning.
Nature.
Philosophical Magazine.

AMERICAN.

Electrical Engineer.
Electrical Review.
Electrical World.

Electricity.
Journal of the Telegraph.
Scientific American.
Street Railway Journal.
Western Electrician.

FRENCH.

Annales Télégraphiques.
L'Éclairage Électrique.
L'Électricien.
L'Industrie Électrique
Journal de Physique.
Journal Télégraphique.

GERMAN.

Annalen der Physik und Chemie.
Beiblätter zu den Annalen der Physik und Chemie.
Electrotechnischer Anzeiger.
Electrotechnische Zeitschrift.
Verhandlungen des Vereins zur Beförderung des Gewerbfleisses.
Zeitschrift für Elektrotechnik.
Zeitschrift für Instrumentkunde.

ITALIAN.

Giornale del Genio Civile.
Il Nuovo Cimento.

The PRESIDENT: In proposing the adoption of the Report of the Council, I have nothing to add to it, except that I think the Institution may congratulate itself on having been one of the means of bringing the profession into line in carrying on the very important conference which took place recently before the Board of Trade in reference to the revised regulations under the Electric Lighting Acts, 1882 to 1890, and which has had such satisfactory results. I beg to move—"That the Report of the "Council, as just now read, be received and adopted, and that it be "printed in the Journal of the Proceedings of the Institution."

Professor JOHN PERRY: I beg to second the motion.

The PRESIDENT: Before putting the motion, I shall be happy to hear any remarks which members may desire to make thereon.

Mr. J. S. RAWORTH addressed the meeting on several points bearing on the existing practice of the Institution in regard to various matters, and expressed his desire to move a resolution—not, however, as an amendment to the motion for the adoption of the Report of the Council.

The motion for the adoption of the Report was therefore put, and carried unanimously.

Mr. RAWORTH then continued his remarks, and read the terms of the resolution which he desired to submit, but which was declared by the President (the Honorary Solicitor so advising) to be out of order, as being connected with the direction and management of the concerns of the Institution—a subject beyond the scope of the Annual General Meeting as prescribed by No. 55 of the Articles of Association, and one to be dealt with by a Special General Meeting of Members and Associates, as prescribed by Article No. 60.

Mr. Raworth's motion, therefore, was not put, but by consent of the meeting the points raised by him were discussed, the speakers being Messrs. A. E. Mavor, Campbell Swinton, H. W. Handcock, H. E. Harrison, J. N. Shoolbred, G. L. Addenbrooke, R. S. Erskine, and J. H. McLean.

Major CARDEW, R.E.: I am asked to propose the following resolution, which I do with great pleasure, viz.:—"That the "members of this Institution desire to thank most cordially the "President, Council, and members of the Institution of Civil "Engineers for their kindness and liberality in again granting "the use of their Lecture Hall for the use of this Institution." It is not necessary, I am sure, for me to say much in support of the resolution; it must obviously commend itself to everyone here.

Mr. J. N. SHOOLBRED: I beg to second the motion.

The resolution was carried by acclamation.

Mr. W. H. PREECE: I have to propose—"That the thanks of "this Institution are due to the Local Honorary Secretaries and "Treasurers for their kind services during the past year."

Mr. A. T. SNELL: I beg to second that.

The resolution was unanimously agreed to.

Mr. H. EDMUNDS: I feel very much honoured in being asked to move—"That the best thanks of this Institution are due to "Sir David Salomons, Bart., Vice-President, for his very kind and "valuable services as Honorary Treasurer." I am sure that all the members of this Institution who know the good work that Sir David has performed in his capacity of Honorary Treasurer

will very readily fall in with this motion; and I certainly have great pleasure, on behalf of myself and of a group of my friends, in bringing it forward.

Mr. CAMPBELL SWINTON: I beg to be allowed to second the resolution.

The resolution was cordially agreed to.

Mr. W. B. ESSON: I have much pleasure in moving—"That "the thanks of this Institution are due to Mr. F. C. Danvers and "Mr. Augustus Stroh for their kind services as Honorary Auditors "during the past year."

Mr. COCKBURN: I have much pleasure in seconding the resolution.

The resolution was unanimously agreed to.

Mr. RAWORTH: I wish to propose the following resolution:— "That the best thanks of this Institution are due to Messrs. "Wilson, Bristows, & Carpmael for their very kind services as "Honorary Solicitors." I make that proposition with very great pleasure. I am one of those people who can appreciate a solicitor even when he does not tell me exactly what I want to hear.

Mr. MAVOR: May I have the honour of seconding the resolution?

The resolution was agreed to unanimously.

Mr. G. L. BRISTOW: I am very much obliged to you, on behalf of myself and my partners. I hope it will not be thought that I had anything to do with stopping the discussion to-night; I merely interpreted your rules. If it is an untoward circumstance that the discussion or any voting upon it could not take place to-night, the rules are responsible for it.

The PRESIDENT: I am afraid it is too late for the discussion to proceed on the two papers which were read at the last General Meeting, which fact I very much regret.

I now have to announce that the scrutineers report the result of the ballot for Council and Officers for the year 1896 to be as follows:—

President:

DR. JOHN HOPKINSON, M.A., F.R.S.

Vice-Presidents:

Sir DAVID SALOMONS, Bart., M.A.
Sir HENRY MANCE, C.I.E., M. Inst. C.E.
ROBERT KAYE GRAY, M. Inst. C.E.
Professor S. P. THOMPSON, D.Sc., F.R.S.

Ordinary Members of Council:

Major A. H. BAGNOLD, R.E.
FRANK BAILEY.
G. VON CHAUVIN.
HENRY EDMUNDS.
W. B. ESSON, M. Inst. C.E.
S. Z. DE FERRANTI.
W. E. LANGDON.
Professor J. A. FLEMING, M.A., D.Sc., F.R.S.
Professor A. B. W. KENNEDY, F.R.S., M. Inst. C.E.
DANE SINCLAIR.
AUGUSTUS STROH.
J. W. SWAN, F.R.S.

Associate Members of Council:

Captain W. P. BRETT, R.E.
H. W. MILLER.
SYDNEY MORSE.

Honorary Auditors:

FREDERICK C. DANVERS.
AUGUSTUS STROH.

Honorary Treasurer:

Sir DAVID SALOMONS, Bart., M.A., Vice-President.

Honorary Solicitors:

Messrs. WILSON, BRISTOWS, & CARPMAEL, 1, Copthall Buildings, E.C.

The PRESIDENT: I have further to announce that the following candidates have been duly elected:—

Foreign Member:

J. S. Rasmussen.

Member:

Albert de Linde, A.M. Inst. C.E.

Associates:

Harry Walton Appleby.
Walter Charles Bersey.
Walter Binns.
Thomas Blackburn.
Lionel E. Buckell.
James Watson Christie.
Frederick Byerley Hobler.
Alfred Henrick Jackson.
Thomas King.
Samuel George Maddison.
Arthur Pettman Patey.
Arthur Pearson.
Thomas Abraham Prout.

Students:

Arthur James Abraham.
Cecil Barber.
George Berry.
John Patrick Clark.
Reginald Charles Clinker.
Arthur F. R. Curteis.
Charles F. Dyer.
W. E. Ferguson.
Robert Fleetwood Fuller.
Herbert Douglas Hodges.
Nathaniel Hudson Howard.
John Vincent Moinet.
William Reginald Potter.
Sydney George Redman.
Charles Renton.
Leonard Graham Stanger-Leathes.
John Gillard Stapleton.
John C. Vaughan.

On the motion of the PRESIDENT, a vote of thanks was accorded to the scrutineers.

The meeting then adjourned.

ABSTRACTS.

E. VAN AUBEL—ON THE HALL PHENOMENON, AND THE MEASURE OF MAGNETIC FIELDS.

(*L'Éclairage Électrique, Vol.* 3, *No.* 22, *p.* 398.)

The author refers to a paper by A. Kundt (*Annalen der Physik*) on the Hall phenomenon in iron and cobalt.

Very thin deposits of iron, nickel, and cobalt were obtained by electrolysis on platinised mirrors, with the object of comparing Hall's discovery with the electromagnetic rotation of the plane of polarisation of light in these metals. Kundt further endeavoured to obtain thin layers of bismuth by electrolysing a solution of tartarate of bismuth, tartaric acid, and ammonia. The deposit so obtained was, however, unsatisfactory.

M. Righi, in his researches on the Hall phenomenon in bismuth (*Journal de Physique*, 2nd series, vol. iii., p. 127, 1884), succeeded in obtaining thin layers of the metal in two ways—firstly, by melting bismuth on a slip of glass, and afterwards thinning the metal down; secondly, by electrolysing a solution of nitrate of bismuth. M. Leduc (*Journal de Physique*, 2nd series, vol. x., p. 112), in his researches on the effect of a magnetic field on the resistance of bismuth, obtained the metal by electrolysing the acid nitrate or ammoniacal citrate of bismuth.

The metallic deposits used in the author's researches were obtained by electrolysis on platinised glass mirrors, the solution employed being either acid nitrate of bismuth or a double tartarate of bismuth and potassium. The thin platinum mirror is cemented to a plate of mica. The ends of four wires are soldered to the bismuth, and secured to the mica.

As it was not found possible to solder the vertical electrodes on two equipotential lines, the current produced through the D'Arsonval galvanometer from this cause was compensated for by means of resistances and a Daniell cell.

The magnetic field was produced by an electro-magnet the poles of which were shaped to give the most intense field. The following are the conclusions arrived at from the results of these experiments:—

1. The electrolysis of acid nitrate of bismuth, or of double tartarate of bismuth and potassium, produces deposits which adhere strongly to a platinised mirror electrode.
2. The electrical resistance of bismuth, obtained by electrolysis from acid nitrate of bismuth, increases considerably in the magnetic field, and the thin layers obtained give rise to the Hall phenomenon in a very marked manner, in the case of both continuous and alternating currents.
3. The deposits obtained by electrolysis from the double tartarate of bismuth and potassium do not give rise to the Hall phenomenon.

4. The deposits of bismuth as obtained by the above process can be obtained without difficulty, and are thinner than those obtained by other processes. This thickness can, moreover, be varied.
5. The Hall phenomenon observed in these thin layers of bismuth forms a very sensitive measure of the magnetic field.
6. The Hall phenomenon can also be employed for measuring one component of the earth's magnetism, by employing sufficiently thin layers of bismuth.
7. Kundt found, experimentally, that the rate of propagation of light through metals is proportional to their electric conductivity; bismuth appears to form an exception to this law. Kundt remarks that the bismuth used by previous workers presented a crystalline structure, whereas the thin layers employed for measuring the index of refraction, presented, even under the microscope, no trace of crystalline formation.

Mr. E. H. Hall, in connection with researches on this subject, remarks that, according to the explanation of the phenomenon offered by E. von Lommil, the Hall current must have a different direction in magnetic metals than in diamagnetic metals, whereas in reality iron and nickle behave in the opposite manner.

The following values of the rotatory power obtained from experiments by M. A. von Ettingshausen and W. Nernst on very pure substances are still more conclusive:—

Diamagnetic Metals.	K.	Magnetic Metals.	K.
Tellurium	+ 530	Iron	+ 0·0113
Bismuth	− 10·1	Nickel	− 0·0242
Antimony	+ 0·192	Cobalt	+ 0·00459

EDOUARD BRANLEY—ELECTRICAL RESISTANCE AT THE CONTACT OF TWO METALS.

(*L'Éclairage Électrique, No.* 18, *Vol.* 3, *p.* 230.)

It is usually considered that the joint produced by two plane surfaces of the same or of different metals lying close together does not offer any appreciable resistance to the flow of an electric current normal to that surface.

The following experiments were made with the object of showing that in some cases the contact resistance between two metals may become appreciable:—

Thin metal plates 48 mm. square were superposed one above the other, after having been carefully cleaned with emery paper. The top and bottom plates were of brass, to which the necessary connections had been soldered, and good contact ensured between the plates by placing a heavy weight above them.

The first experiment consisted in placing two copper plates and a zinc plate between the brass ones.

An ohm resistance was connected in series with the plates in order to make the measurement more sensitive. The resistance of the plate contacts, as measured

by a Wheatstone bridge, under the above conditions was found to be extremely low, and to keep constant for several days.

In the second experiment, the copper plates were replaced by bismuth, and the zinc by aluminium. In this case the contact resistance was found to be appreciable, and it was not found necessary to connect an auxiliary resistance in series with the plates. It was, moreover, noticed that a marked increase in the resistance takes place with time, as will be seen from the following figures:—

H.	M.	Resistance in Ohms.	H.	M.	Resistance in Ohms.
9	20	0·4	10	55	1·7
9	45	0·74	2	0	3·005
10	0	0·83			

This resistance cannot be attributed to an effect due to polarisation, for on connecting the plates direct to the galvanometer no deflection is obtained.

In the third experiment, two bismuth plates are placed together, then two brass plates, and above these two aluminium plates. In this case the resistance was found to be extremely low, and it was found necessary to employ an auxiliary series resistance for making the measurements.

The conclusion which the author arrived at from these experiments is that the contact resistance between two such metals as copper and zinc is inappreciable.

In the case of other couples, such as lead and aluminium, lead and iron, tin and aluminium, bismuth and iron, bismuth and aluminium, &c., there is a resistance due to contact.

Its observed initial value depends on the nature of the metals, and also on the rapidity with which balance is obtained on the Wheatstone bridge. The resistance decreases rapidly at first, and slowly afterwards.

The contact resistance is reduced by increasing the pressure between the plates. Even when the plates were screwed up in a vice the resistance was found to increase with time. On some occasions when the circuit from the Daniell cells was broken, so that no current could possibly pass through the plates, the resistance was still found to increase.

In most of the tests the plates were cleaned directly before the experiment, but the same phenomena were observed with plates which had been cleaned several days previously, and also with plates whose surfaces had become oxidised.

The contact resistance between two metallic surfaces as observed above is altered by mechanical shocks and electric sparks.

It is found that mechanical shocks in the neighbourhood of the plates has the effect of increasing the contact resistance. Electric sparks will reduce the contact resistance, the effect being, however, small. An appreciable decrease is observed when one part of the plates is connected to a weakly charged Leyden jar. In the case quoted above (bismuth and aluminium) where the resistance altered from 3·005 to 0·45, such an alteration may last for some time, but it has been noticed that mechanical shocks will greatly help to restore the resistance to its original value.

A. LEMOINE—ON THE MEASUREMENT OF VERY HIGH POTENTIALS.

(L'Éclairage Électrique, Vol. 3, No. 23, p. 433.)

No great difficulty presents itself in the measure of very high potentials, except for the necessity of maintaining the potential constant for the period during which the reading is taken. This end is attained by employing an overcharged system obtained by establishing a permanent discharge between two conductors from an electrostatic machine. Under these conditions the variations of potential were found not to exceed one in one thousand.

Two instruments were designed by the author—one destined to be used as a standard electrometer, and the other being a simplified form. They have a range of 1,000 to 100,000 volts, the most favourable range being from 5,000 to 40,000 volts.

The standard pattern consists of a balance, with plane disc and guard ring constructed on Lord Kelvin's principle. The arms of the balance beam are short, and the oscillations limited by adjustable stops. The balance is mounted on a solid brass table, fixed to four metallic rods, and 28 centimetres above the base of the instrument. A circular aperture in this table allows the attracted disc to pass through. The diameter of this disc is 5·95 centimetres, which gives an attractive force of 5 grammes for a distance of 1 centimetre between the plates, when the potential is 10,000 volts.

The movable disc is of aluminium, and is balanced by a counter-weight suspended on the other arm of the balance.

The base of the instrument, the supporting columns, the disc, and guard ring are all connected to earth. Under the attracted disc is placed an adjustable horizontal plate of the same diameter as the aperture in the guard ring. This plate is insulated, and is connected to the conductor whose potential it is desired to measure. The plate is mounted on its supporting column by means of a ball-and-socket joint used for adjusting it parallel to the disc.

The necessary insulation is obtained by means of a shellacked glass rod 7·5 centimetres long.

The lower plate has a travel of about 5 centimetres, and its displacements are measured in the usual manner by means of a vernier. This instrument has an error of 1 in 1,000.

The standard type being too delicate for some purposes, a simplified type was designed for cases where an error of 1 in 100 would not be too great.

The general design is almost similar to that of the standard pattern. The balance employed is a small Roberval balance of the commercial type.

In order to obtain the necessary insulation without inordinately lengthening the glass rod, a large glass disc is cemented to the base of the glass rod, thus allowing the instrument to be used for potential differences of 100,000 volts. The metal supporting columns are covered with glass tubes. For potentials above 50,000 volts it is necessary to place a sheet of glass over the lower electrified disc. Notwithstanding the strong construction of the instrument, and that the readings are made in tenths of a millimetre, and with weights of 1 centigramme, it is possible to measure up to 100,000 volts, with an error not exceeding 1 in 1,000.

J. REYVAL—THE ELECTRIC LIGHTING OF THE KIEL CANAL

(L'Éclairage Électrique, Vol. 3, No. 25, p. 529.)

Some difficulty was experienced in the electric lighting of the Baltic Canal, chiefly in regulating a series system over very long distances. Apart from the canal itself, it was also necessary to light the locks, docks, buildings, and light-houses situated in the neighbourhood.

The general scheme consisted in—

1. The installation of two generating stations—one at Holtenau, and the other at Brunsbüttel.
2. The use of machines and apparatus working under the safest and most economical conditions. A complete set of spares.
3. The maintaining of a constant potential at the two stations at all loads.
4. The absolute independence of each lamp, so that the extinction of one portion of the installation would not influence the rest.
5. The exclusion of all doubtful expedients, such as main circuit regulators and oil insulation. Each of the two stations contain two sets of slow-running direct-coupled plants, on the Helios system. Each set delivers 200 H.P. when running at 200 revolutions per minute. The two tandem cylinders are 400 and 620 mm. in diameter, and have a 1-metre travel.

The alternator is mounted between the two bearings, and of which the field magnets form the fly-wheel. The rim of the wheel is turned, and to which are fixed 72 magnet poles built up of iron plates ½ mm. in thickness.

The diameter of the field magnets is 4·752 metres, the peripheral speed 20·1 metres per second, and the frequency 51.

The armature is so built that the whole ring can be slid to one side and the top half lifted off, to allow of easy inspection. The armature cores are built up so as to form as closed a magnetic circuit as possible. A section of the armature core, with its coil, can be withdrawn by merely undoing four screws.

The E.M.F. of the alternators is 2,000 volts. These alternators are excited from four-pole shunt-wound disc armature machines, mounted on the end of the main shaft, the exciting voltage being from 120 to 150 volts. The exciting current is automatically regulated by means of a Tesla motor, which introduces resistances in the field circuit of the exciter.

The normal working steam pressure is 6 atmospheres, and with an output of 100 kilowatts the steam consumption is 12·5 kilogrammes per kilowatt-hour.

A direct-coupled 12-H.P. day-load set is installed at the Brunsbüttel station for the lighting of certain buildings. It is a four-pole disc dynamo, which can in case of need be used for exciting the alternators.

The insulation on all the apparatus consists of two thicknesses of ebonite and one of porcelain, and was tested up to 10,000 volts. The measuring instruments are mounted on white marble and ebonite. Auxiliary resistances, made of nickel steel wires, are used for putting the machines in parallel. In addition to the ordinary synchronising transformers, there is also employed another device to help in synchronising the machines This consists of a movable disc placed before

two electro-magnets, each excited from one of the machines. When the two machines are in phase the disc remains stationary, but when they are out of phase the disc rotates; and the direction of rotation indicates which machine is in advance.

The canal circuit of 98 miles is divided into four sections. One section leaves Holtenau, and is 47 miles long, following the north and south banks of the canal. From this point there are two branches which meet at Brunsbüttel. From the latter place the go and return circuit is 99·5 kilometres long on the north side of the canal, and 90·8 kilometres on the south side. At the Holtenau end the north bank is 98·6 kilometres, and the south 97 kilometres long. Each section supplies 250 lamps of 25 candle-power, making a distance of 199 metres from lamp to lamp.

For the high-tension overhead conductors 4 mm. diameter copper wires are employed. These are supported on special porcelain insulators fixed to poles placed 40 metres apart. Above each lamp is placed a small choking coil, the winding of which is connected in series with the overhead conductor, and to the terminals of which the incandescent lamp is connected, the latter being therefore in parallel with the coil. Should the lamp circuit be broken, this will therefore not influence the main circuit. The choking coil is designed only to waste 9 per cent. of the power required by the lamp. More than one-third of the 250 lamps in each circuit can be extinguished without necessitating any regulation.

The difference of potential at the terminals of each lamp is 25 volts, which therefore necessitates 7,500 volts for the whole circuit. This is obtained by means of step-up transformers of the Helios type.

The overhead conductors are protected against lightning by means of a barbed wire fixed parallel to them along the tops of the poles.

The armoured cables passing under the canal are immersed to a depth of 1 metre in the bed of the canal, and consist of two conductors insulated with rubber having a gutta-percha covering. The armouring consists of galvanised iron wires. The cables were tested up to 15,000 volts. During the above installation an electric launch was employed having a speed of 15 kilometres per hour, and capable of covering the whole length of the canal in $7\frac{1}{2}$ hours.

M. MORISOT—ON A CELL OF HIGH AND CONSTANT E.M.F.

(*L'Éclairage Électrique, Vol. 4, No. 33, p. 297.*)

The cell is made up in the following manner:—

1. The positive pole consists of a plate of retort carbon dipping in the depolarising liquid. The latter is contained in the outer jar, and consists of one volume of sulphuric acid mixed with three volumes of a saturated solution of bichromate of potash. Crystals are added to the solution to keep it saturated.
2. A porous pot immersed in the above depolarising liquid contains a solution of caustic soda (density about 1·05).
3. The plate of amalgamated zinc forming the negative pole is immersed in a second porous pot placed within the above, and containing a saturated solution of caustic soda (density about 1·25).

The E.M.F. of this cell is 2·5 volts at the start, and keeps constant above 2·4 volts during at least 10 hours of uninterrupted work.

The internal resistance is about 0·8 ohm, but this varies with the nature and thickness of the porous pots. In experiments made by the author on a cell containing 600 cm.^{3} of depolarising liquid, 130 cm.^{3} of weak soda, and 110 cm.^{3} of concentrated soda solution, a current of 0·432 ampere was maintained through a resistance of 5 ohms, and of 0·220 ampere through a resistance of of 10 ohms.

The increase of 0·4 volt obtained with this cell over the ordinary bichromate cell is due to the substitution of the alkaline solution for the acid solution usually surrounding the zinc.

The same substitution made in the Daniell or Bunsen cell will also give an increase in the voltage.

The advantage obtained by this substitution alone is small, owing to the rapid increase of resistance. The soda is converted into a neutral chromate, and one finds on the zinc an abundant deposit of zinc hydroxide. This action is, however, greatly checked by the interposition of the weak alkaline solution. This addition does not greatly increase the resistance, and it offers the advantage of ensuring an almost constant electro-motive force, especially if the intermediate solution be renewed from time to time.

The use of potassium instead of sodium does not offer any advantage, neither does the use of bichromate of sodium instead of bichromate of potassium in the depolarising liquid.

The zinc is attacked far less than in the Poggendorff cell, or any other in which the zinc dips in acidulated water.

After 10 hours of continuous action, the zinc is found to be covered with a grey coating. This can be removed in a few moments by dipping the zinc in acidulated water. Under these conditions, and even without having renewed the liquids, the cell will be in normal working order.

M. DUEZ—A COMPARISON BETWEEN ELECTRO-MOTORS FOR CONTINUOUS CURRENTS AND FOR POLYPHASE CURRENTS.

(*Comptes Rendus, Vol.* 121, *No.* 3, *p.* 160.)

The analogy between continuous-current and polyphase-current motors is noticed in the formulæ. For instance, in both cases the expression for the torque is $W = N_2 I_2 \phi$, and for polyphase motors $N_2 \omega_1 \phi = I_2 R_2 + N_2 \omega_2 \phi$. This is the same as with a continuous-current motor, the difference of potential of which would be $\omega_1 \phi N_2$, and the counter E.M.F. of which would be equal to $N_2 \omega_2 \phi$.

It is interesting to note that in transmission of power the polyphase motor acts both as a continuous-current motor and as a transformer. If we consider the generator as consisting of two rectangular frames displaced in a fixed field ϕ, the tongue necessary to turn the generator will be $I \phi'$.

Then, $$I_1 \phi' \omega_1 = I_1^2 R_1 + \phi \omega_1 I_2 ;$$
or $$\phi' \omega_1 = I_1 R_1 + \phi \omega_1 \frac{I_2}{I_1}.$$

The conditions are as though the generator was a continuous-current dynamo, having a difference of potential of $\phi\ \omega_1 \frac{I_1}{I_2}$ at the extremities of the line, this difference giving by transformation a difference of potential $\phi_1\ \omega$ at the terminals of the motor.

The author next considers in which case this transformation offers an advantage over continuous currents. For the same loss in the line and same weight of copper in the motor, under what condition is the torque greater in the polyphase- than in the continuous-current motor?

With polyphase currents, $W = N_2\ I_2\ \phi.$

But, as $\phi = \sqrt{N_1^2\ I_1^2 - N_2^2\ I_2^2},$

one obtains $W = N_2\ I_2 \sqrt{N_1^2\ I_1^2 - N_2^2\ I_2^2};$

and for the same value of I the torque is a maximum when

$$N_2^2\ I_2^2 = N_1^2\ I_1^2,$$

and this maximum value is $W = \frac{N_1^2\ I_1^2}{2}.$

In the continuous-current motor the torque is approximately equal to $N_1\ N_2\ I_1^2$; the ratio of the two torques will then be $\frac{N_1}{2\ N_2}$.

With the same number of turns in the two motors, the former will have the advantage over the latter motor with respect to torque, when the ratio $\frac{N_1}{2\ N_2}$ is very great.

The author remarks that the formula $\phi'\ I_1 = \frac{I_1^2\ R_1}{\omega_1} + \phi\ I_2$ shows that if the loss in the line is very small with respect to the number of turns in the generator, then the torque required for the generator is sensibly equal to the torque produced by the motor.

M. MAURAIN—VIBRATIONS OF A TUNING FORK IN A MAGNETIC FIELD.

(Comptes Rendus, Vol. 121, No. 5, p. 248.)

When a tuning fork is placed in a magnetic field its rate of vibration is modified, and the extent to which this takes place depends on its orientation in the field.

The author made three series of experiments. In the two first ones the axis of the tuning fork was perpendicular to the field, and so placed that the vibrations were either parallel or perpendicular to the field, the latter being produced by a Rhumkorff electro-magnet. In the third series the axis of the tuning fork was parallel to the field, in which case the tuning fork was placed in the interior of a solenoid. The vibrations were compared, by inscription, to those of another fork placed out of the field.

The following is a list of ratios of these two readings:—

1. *Axis perpendicular, and plan of vibration parallel to the field.*

Strength of current in amperes in the magnetising coil	0	2	4·1	7·8	12	16·7
Ratios	1·8327	1·8249	1·8158	1·7873	1·7748	1·7624

This, then, shows a reduction of 3·8 per cent. in the rate of vibration with the strongest field—about 6,350 C.G.S. units.

2. *Axis and plan of vibration perpendicular to the field.*

Strength of current	0	4·9	9·6	17·25	11·8
Ratios	1·8387	1·8396	1·8407	1·8460	1·8526

Here the number of vibrations increases with a field of 6,530 C.G.S. units.

3. *Axis parallel to the field.*

Strength of field ...	0	225	427	503	697	787	1,090
Ratios	1·8350	1·8361	1·8371	1·8380	1·8393	1·8402	1·8420

The number of vibrations increases by 0·38 per cent. in a field of 1,090 units.

In the first set of experiments the author noticed a marked effect due to hysteresis when the current was varied through a cycle; the diminution in the number of vibrations for the same current being greater when descending than when ascending.

The figures given above are from experiments in which the current was progressively increased from zero to the maximum value; the following are the results obtained when the current first increased to a maximum, then reduced to the required strength :—

Strength of current	0	2	4·1	7·8	12
Ratios	1·8308	1·8224	1·8076	1·7861	1·7726

This effect is probably not due to the tuning fork itself, the vibrations preventing all retardation of magnetisation. It may be due either to a retardation in the action of the field on the elasticity of the steel, or to a retardation in the magnetisation of the armatures of the electro-magnet, or perhaps to the two causes combined. The damping of the vibrations is greater as the intensity of the field increases, this being chiefly due to Foucault currents, which oppose motion.

The author also made experiments with a strip of brass vibrating transversely in the electro-magnet, but could detect no effect due to the magnetic field.

H. DESLANDRES—SPECTRUM ANALYSIS OF THE CARBONS OF AN ELECTRIC FURNACE.

(*Comptes Rendus, Vol.* 120, *No.* 23, *p.* 1259.)

M. Moissan recently discovered that the carbons of an electric furnace undergo a purifying action with the passage of very strong currents through them, and are at the same time converted into graphite. It is difficult to obtain pure carbon by chemical means. This property of the electric furnace is therefore interesting to those who frequently employ carbon electrodes for qualitative analytical researches. The author has therefore carried out this investigation with a view to study the complete carbon spectrum. The two pieces of carbon employed were 0·20 inch long and 0·05 inch wide Small pieces of carbon were removed from each pole at variable distances from the arc (0·15. 0·10, 0·05, 0·01). The pieces farther from the arc show lines representing the ordinary impurities of carbon, such as alkaline earths, copper, iron, and silicon; but when taken nearer the arc

the lines of impurities gradually diminish, and finally disappear. Calcium must however, be excepted, the lines of which, although reduced, are always visible; this being due to the proximity of the furnace lining, which is partly volatilised with very intense currents.

The purification of carbon seems due to a purely physical cause; foreign matters much more volatile than carbon are liberated in the form of vapour. The purest portion of the two poles are the mushrooms which form on the negative pole. It is with one of these mushrooms that the following carbon spectrum was obtained, and which contained fewer lines than similar spectra published by Livieng and Dewar, Hartley and Adeney, Eder and Valenta:—

Intensity.	Wave-Length.	Intensity.	Wave-Length.
8	426·7	4	274·75
5	392·17 }	3	264·12
4	391·17 }		
2	316·83 ? }	8	251·19
1	316·57 ? }		
2	299·34	8	250·79
1	296·77	10	247·88
8	283·75	8	229·7
8	283·64		

This table only includes photographic waves with ordinary plates and length—waves from 480 to 220.

M. BALLAND—ON ALUMINIUM UTENSILS.

(*Comptes Rendus, Vol.* 121, *No.* 9, *p.* 381.)

Since the experiments on aluminium published by the author in June, 1892, utensils made of this metal have been largely used in the French Army. These are made without any soldered joints, by stamping them.

It is found the weights of these different articles, made under the same conditions, from sheets of the same gauge, are not as uniform as they should be.

It is discovered that this is due to their being washed with soda. Aluminium is actively attacked by soda or potash. In a hot 20 per cent. solution of soda the aluminium will lose 10 to 20 per cent. of its weight in a few seconds, and disappear after a few minutes, leaving only a blackish deposit.

When ordinary water is left for several months in aluminium utensils, it is noticed that white blotches form at irregular places. A few centigrammes of the white powder can be obtained by desiccation. The spots are specially to be noticed wherever the metal contains particles of iron, silicon, carbon, or soda; and form specially round rivets, the latter consisting of aluminium alloys. The loss in weight after six months does not amount to 0·1 gr. per cent.

Some slips of aluminium which the author kept immersed in Seine water for four years, were found to be covered with a coat of fine oxide, which could not be removed with brickdust, but which disappeared without difficulty when the slips were immersed for 24 hours in a 1 per cent. solution of sulphuric acid. The slips, after cleaning, were found to have lost 3 per cent. of their weight, and presented a bright appearance, except in several places where the metal was badly corroded.

With a saline solution containing 35 grains of salt per litre the same effects are noticed as in ordinary water, but to a greater degree. The metal is attacked more deeply. The action is greatest at the level of the liquid, where both air and water act together. The loss of weight after four months was 6 per cent. The effect of sea water, containing about 35 grains of salt per litre, would no doubt be greater.

It is also noticed that when left for some time in a saline solution the aluminium swells in places, owing to the liquid reaching impurities of iron and silicon through the metal.

A vessel containing vinegar will after several months have a white ring on the exterior at the same level as the vinegar inside. This is not produced with ordinary water, salt water, sulphuric acid 1 per cent.

The author does not consider that the future use of aluminium will be influenced by the above effects, as they may in most cases be attributed to the presence of foreign metals contained in commercial aluminium. Most aluminium objects are made of aluminium alloys, or of aluminium containing up to 8 per cent. of unequally distributed impurities, such as iron, silicon, alumina, nitrogen, carbon, and borate of carbon.

The chief efforts in the industry will be in future to give the metal a close, even texture, and thoroughly homogeneous, and also a polished surface. It will also be necessary to avoid the use of soda for cleaning the metal, as this makes the surface matt, and renders it more likely to be attacked. All foreign metals should be avoided in soldering, &c.

H. BORDIER — A NEW METHOD OF MEASURING ELECTRIC CAPACITIES, BASED ON THE SENSITIVENESS OF THE SKIN.

(*Comptes Rendus, Vol.* 121, *No.* 1, *p.* 56.)

The principle of this method depends on one of M. D'Arsonval's experiments.

If one connects a set of condensers in parallel to an induction coil, and then notices the moment of minimum sensation produced by the current on the skin. either by means of a rheostat, or by displacing the coil along a divided scale. it will be found that this moment will vary for each increment of capacity. If, for instance, a microfarad is divided into tenths, it is easy to find the positions either of the rheostat or of the coil which correspond to the initial sensations produced by each tenth of a microfarad which is added. If a curve be plotted with the capacities as abscissæ, and the different positions of the rheostat or of the coil as ordinates, this will represent the variation of cutaneous sensitiveness with different values of capacity. It will then only be necessary to note the moment at which the faradaic current produces the sensation, the coil being connected to the condenser whose capacity one wants to measure. By referring to the above curve one would find at once the value of this capacity.

In order that conditions may remain the same throughout an experiment, it is well to dip the fingers into two vessels containing water and connected to the two terminals of the induction coil.

The following is an example of the results obtained by this method.

The coil employed had a resistance of 668 ohms; the wire had a diameter of 0·2 mm., and a length of 1,300 metres.

To begin with, the position of the coil is sought for, corresponding with the minimum sensation, and is found to be 19·25. A microfarad condenser divided into tenths is then connected in parallel, and the following positions of the coil correspond to the minimum sensations:—

Added Capacity in Microfarad.		Positions of the Bobbin. Cm.	Added Capacity in Microfarad.		Positions of the Bobbin. Cm.
0	...	19·25	0·6	...	12·5
0 1	...	16·75	0·7	...	12
0·2	.	15·5	0·8	...	11·6
0·3	...	14·5	0·9	...	11·25
0·4	...	13·75	1·0	...	11
0·5	...	13			

Two condensers measured by this method gave a = 0·915 microfarad, and b = 09·1 microfarad. The ballistic galvanometer method gave a = 0·912, and b = 0·915 microfarad.

The author states that the sensitive effects are not always diminished when the capacity of the coil is increased; if this consists of a large wire of low resistance, it is found that the sensation increases with an increasing capacity.

This method enables one to make a very approximate measurement of the capacity of the human body.

The latter is connected in series with one of the wires connecting the coil to the standard condenser.

Capacities added to the Coil. Microfarad.		Variations of the Coil. 1st Experiment. Cm.		2nd Experiment. Cm.
0	...	20·00	...	19·25
0·1	...	17·75	...	16 75
0·1 + the body		17·7	...	16·7
0·02	...	16·75	...	15·5

The mean of the two experiments gives 0·0025 microfarad as the capacity of the human body.

This capacity is about 58 times greater than that of a homogeneous conductor having a surface equal to that of the body.

E. BOISTEL—THE STEINMETZ MONOCYCLIC SYSTEM OF DISTRIBUTION.

(L'Éclairage Électrique, Vol. 3, No. 19, p. 259.)

The monocyclic system of distribution has been advocated by Mr. Louis Bell specially for use on the numerous American alternating-current circuits working at the present time, and enabling polyphase motors to be used to their full advantage. This paper includes a table comparing the different alternating-current systems with their relative weights of copper.

The weight required for the simple two-wire alternating-current system is

taken as 100. With the three-wire system having the section of the middle wire half that of the outer wires the weight is 31·25. Motors cannot, however, be worked well from either of these systems. With the diphase system having two distinct circuits the weight is 100. The disadvantage of this system is the difficulty of regulating the two circuits. The weight required for the diphase system with three wires depends on the limits given to the P.D. If the maximum pressure is limited to that of the two-phase system, then the weight of copper is raised to 145·5, as the third wire has the effect of producing a quarter-period difference of phase in the two circuits. But if the same voltage exist between the middle and outer wires as in the four-wire system, then the weight falls to 72·8. This system allows the use of both lamps and motors, and eliminates the difficulty of regulating two distinct circuits.

In the case of the three-phase system with three wires, with the same working pressure, the weight of copper is exactly three-fourths of the weight with the ordinary two-wire system, viz., 75. The P.D. on the three circuits varies with alterations in the load, on account of reactions in the transformers and generator. With respect to the self-induction of the line, the system is symmetrically balanced.

When four wires are employed on the three-phase system a great economy of copper is obtained, the fourth or compensating wire being connected to the junction of the three phases. The lamps are connected between the compensating wire and any of the three wires. This virtually corresponds to two lamps connected in series with a quarter-phase difference between them, or 1·73 times the volts required by one lamp.

The weight of copper is 29·2. assuming that the compensating wire has a section half that of the others.

The monocyclic system has the advantage of not possessing the complications inherent in a polyphase system, and yet being capable of working motors satisfactorily. This system necessitates the use of an additional winding on the armature of the generator, connected to a point in the centre of the main winding, and so placed as to produce the necessary difference of phase with the latter. To this auxiliary winding is connected a third wire of small section, which is taken to points where motors are installed.

An advantage of this system is that the generator can be compounded for losses in the line.

For running a motor two transformers are connected to the middle and outer conductors, and in the secondary of which the necessary difference of phase will exist for running the motors.

It is possible to instal a monocyclic machine in a sub-station for working motors in the vicinity; and in this case the motor wire would only start from the sub-station. The section of the motor wire need not be greater than a quarter the total section of the line, and in many cases a smaller wire than this would suffice. The weight of copper, therefore, works out to 125 on an ordinary two-wire system, and to 39 on a three-wire system. A large number of these generators are working daily in American central stations on the ordinary circuits, and in some cases have replaced the previous machines. The results obtained have proved most satisfactory.

M. J. LAFFARGUE—THE DISTRIBUTION OF ELECTRICAL ENERGY IN PARIS.

(*Bulletin de la Société Internationale des Électriciens, Vol. 12, No. 122, p. 403.*)

Since the commencement of electric lighting in Paris, rapid strides have been made in the various systems of distribution. Considerable modifications have been made in the systems of mains. Serious difficulties were experienced with the system of bare copper strips supported on porcelain insulators. In some cases artificial ventilation of the conduits is resorted to. Lead-covered armoured cables, laid directly in the soil, have given very good results.

Special rules have been issued by a technical committee for the wiring of houses.

The practical behaviour of supply meters is proving satisfactory. The average errors do not exceed 4 to 5 per cent. up or down. Each company employs a special staff for the testing and maintenance of their meters. Electric motors are being largely used for lifts, on account of the high price of hydraulic power.

The following tables are of use in showing the rate at which electric lighting has developed in Paris :—

1890	Total output of the stations,	3,900	kilowatts.
1892	,, ,,	8,670	,,
1894	,, ,,	11,270	,,
1895 (end) ...	,, ,,	18,945	,,

In the latter figure are included all stations in course of erection.

PARIS CENTRAL STATIONS, OCTOBER, 1895.

——	Total Power in Kilowatts at the Central Station.		Lighting.		Motors.	
	In Use.	In Progress.	10-C.P. Lamps Installed.	Power in Kilowatts.	No.	Power in Kilowatts.
Edison Continental Co. ...	2,400	680	88,000	3,080	30	36·8
Electric Light and Power Co.	2,025	500	70,000	2,450	35	90
Clichy District Co.	2,470	...	95,000	3,500	67	176·6
Paris Electricity and Compressed Air Co. ...	2,200	2,400	80,000	2,800	...	...
Champs-Elysées Co.	1,200	400	80,000	2,800	15	25·7
The Left Bank Co.	100	4,000	...	...	...	...
Municipal Division	570	...	41,567	1,455	7	2
	10,965	7,980	454,567	16,085	154	331·1

The Edison Continental Company has three central stations and one sub-station in Paris. These three stations can be connected in parallel if necessary and any of them is capable of taking up the day load.

A load curve of Oct. 19th, 1895, shows a maximum of 1,680 kilowatts at 5.30 p m At 6 a.m. the load is about 400 kilowatts, owing to the charging of accumulators. In the evening the load falls off rapidly.

The Electric Light and Power Company has an output of 2,025 kilowatts from its several stations. At one of the stations two Weyher-Desroziers sets of 100 kilowatts each are being removed, and a Corliss-Farcot-Desroziers set of 441 kilowatts is being installed, as well as a Laval-Desroziers generator of 220 kilowatts. The total available power will then be 2,525 kilowatts. From a load curve for Oct. 12th–13th, 1895, it is seen that the maximum load is 1,550 kilowatts at 6 p.m. The greatest load obtained was 1,630 kilowatts at 5.30 on 24th–25th Dec., 1894.

The Clichy District Company has considerably increased its output since its establishment. It has at present three horizontal sets of 350 kilowatts, three vertical sets of 350 kilowatts, also three Armington & Sim's engines working 50-kilowatt shunt dynamos for charging accumulators, also one 100-H.P. engine working "Booster" dynamos. The total output is 2,470 kilowatts. The company intend supplying electrical power for working lifts. The maximum load produced was 980 kilowatts at 6 p.m. on Oct. 15th, 1895.

The Paris Compressed Air and Electricity Company commenced operations in 1889; compressed air being employed as a motive power. Two extra stations were built later working at a pressure of 2,000 to 3,000 volts, and feeding into 25 sub-stations for charging accumulators.

Two distinct circuits are employed, passing through each of the sub-stations in order that the circuits may be worked together if required.

The cost of maintenance of so large a number of accumulators was found to be great, and this system did not give satisfaction.

In most cases the accumulators have been replaced by motor transformers of 40 or 80 kilowatts, consisting of two Thury dynamos coupled together and mounted on the same bed-plate. The use of so many sub-stations has not proved satisfactory, and the company intend replacing them with two or three large sub-stations. The Saint Roch station consists of three groups of four transformers connected in series, and feeding into a five-wire system of distribution. Two groups of transformers have an output of 80 kilowatts, and the third of 40 kilowatts. There are also 16 batteries of accumulators of 50 kilowatts. The company is building a new station on the Quai Jemmapes. It will contain three 800-kilowatt 600-volt dynamos for five-wire distribution.

The Champs-Elysées District Company employs alternating currents. The central station at Lerallois-Perret, on the banks of the Seine, contains three Hillariet alternators of 1,200 kilowatts, and is at present installing a Farcot steam alternator.

A mean load curve for October for this station shows that the maximum load of 600 kilowatts is between 7 p.m. and 8 p.m., and at 3 a.m. and p.m. the load is inappreciable.

The Left Bank Company was definitely formed in 1894; it had, however, commenced operations in October, 1893, by using the small Pantheon station of 100 kilowatts capacity. The station now in course of construction at Issy, on

the banks of the Seine, will have an output of 4,000 kilowatts, from 10 Ganz alternators of 400 kilowatts, at 3,000 volts and 42 frequency. The cables will be of the Felten & Guillaume concentric lead-covered, armoured type, insulated with jute and with paper, and manufactured by the Société Industrielle des Telephones at Bezons. Sub-station transformers will be employed, the secondary consisting of bare copper strip run on porcelain insulators in bitumen culverts. A similar system is in operation.

The Municipal Station of the Halles Centrales has not undergone any important alterations since its commencement. The total output is 570 kilowatts, of which 240 kilowatts are for continuous currents, and 330 kilowatts for alternating currents. Four Desroziers 42·5-kilowatt dynamos have been added for charging accumulators, and one 38-kilowatt Patin fly-wheel alternator. The load-factor of this station is nearly even through the day, owing to the lighting of the basements of the markets.

The mean curve of the Paris station for a day in October, 1895, shows that the maximum load of 6,778 kilowatts is at 6 p.m., and the minimum of 550 kilowatts at 7 a.m. The total amount of energy for one day is 45,500 kilowatt-hours.

The yearly increase in the consumption per annum is as follows:—

1889	480,784	kilowatt-hours.
1890	2,284,485	,,
1891	4,671,706	,,
1892	5,987,538	,,
1893	6,986,927	,,
1894	7,883,434	,,

The private installations in Paris amount to 25,760 H.P.

CLASSIFIED LIST OF ARTICLES

RELATING TO

ELECTRICITY AND MAGNETISM

Appearing in some of the principal Technical Journals during the months of JUNE to DECEMBER, 1895, inclusive.

S. denotes a series of articles. **I.** denotes fully illustrated.

ELECTRIC LIGHTING AND POWER.

J. GROSSELIN—On Concentric Mains.—*Bull. Soc. Int.*, vol. 12, No. 119, p. 306 (I.).

E. J. BRUNSWICK—On the Electric Transmission of Power in Workshops.—*Ecl. El.*, vol. 3, No. 23, p. 440, No. 24, p. 492, No. 26, p. 587 (I.).

A. B. HERRICK—Losses in Central Stations and Electric Transmissions.—*Ibid.*, p. 455.

ANON.—The First Three-Phase Transmission in Canada.—*Ibid.*, p. 457.

E. EGGER—Electric Regulator of Speed of Turbines.—*Ibid.*, p. 459.

E. HOEGERSTÄDT—A Special Method of Arranging a Transmission of Motive Power.—*Ibid.*, p. 460 (I.).

ANON.—The Switch-Board of the Baltimore Central Station.—*Ecl. El.*, vol. 3, No. 24, p. 518 (I.).

J. REYVAL—The Illumination of the Baltic Canal.—*Ecl. El.*, vol. 3, No. 25, p. 529 (I.).

E. BOISTEL—The Steinmetz Monocyclic System of Distribution.—*Ibid.*, p. 546.

P. TABOURY—Electric Welding of Metals: The Zerener Process.—*Ecl. El.*, vol. 3, No. 26, p. 590 (I.).

ANON.—A Commutator with Movable Segments.—*Ibid.*, p. 594 (I.).

G. RICHARD—Electric Welding.—*Ecl. El.*, vol. 4, No. 27, p. 14 (I.).

C. F. JENKINS—Regulation of Motors from a Distance.—*Ecl. El.*, vol. 4, No. 28, p. 80 (I.).

A. BOCHET—Efficiency of Electricity Works: American Stations. Electric Distribution of the Line from Sceaux to Paris.—*Ecl. El.*, vol. 4, No. 29, p. 97 (I.).

G. RICHARD—Incandescence Lamps.—*Ibid.*, p. 124 (I.).

ANON.—Three-Phase Current-Transmissions in America.—*Ecl. El.*, vol. 4, No. 31, p. 215.

ANON.—The Steinmetz "Polycyclic" System.—*Ibid.*, p. 217.

ANON.—Electric Lamps for Locomotives.—*Ecl. El.*, vol. 4, No. 33, p. 314.

ANON.—Electric Train Lighting.—*Ibid.*, p. 317 (I.).

A. BOLLINCK—On Electric Power Transmission in Factories.—*Ecl. El.*, vol 4, No. 35, p. 419.

A. WITZ—Lighting by Phosphorescence.—*C. R.*, vol. 121, No. 6, p. 306

ANON.—The Monocyclic System.—*E. T. Z.*, 1895, No. 23, p. 346 (I.).

ANON.—Electric Lighting in Berlin.—*E. T. Z.*, 1895, No. 24, p. 370.

ANON.—Electric Lighting of the Baltic Canal.—*E. T. Z.*, 1895, No. 25, p. 378.

H. GÖRGES—Remarks on the History of the so-called "Monocyclic" System.—*E. T. Z.*, 1895, No. 26, p. 389.

ANON.—Travelling Crane made by the Union Electricity Company.—*Ibid.*, p. 390.

R. ULBRICHT—The Electricity Works of the Dresden Railway Station.—*E. T. Z.*, 1895, No. 27, p. 401. No. 28, p. 435 (I.).

H. VIETZE—Arrangement of Connections for running Alternators in Parallel.—*E. T. Z.*, 1895, No. 28, p. 439.

E. EGGER—On Electric Lifts.—*Ibid.*, No. 29, p. 450 (I.).

ANON.—Development of the Use of Motors in Berlin.—*E. T. Z.*, 1895, No. 30, p. 469.

E. BLATTNER—The Electric Works at La Goule.—*E. T. Z.*, 1895, No. 31, p. 473 (I.).

ANON.—Doubrava & Donat's Arc Lamp.—*Ibid.*, p. 484 (I.).

F. ROSS—The Dubiau Steam Pump; a means of greatly increasing the Output of Boilers.—*Ibid.*, p. 486 (I.).

E. HOLBEN—Gear Wheels and Electric Motors.—*E. T. Z.*, 1895, No. 33, p. 515 (I.).

W. MEISSNER—Electrically Driven Rock Drills, and the Rock-Drilling System of Siemens & Halske.—*E. T. Z.*, 1895, No. 34, p. 537 (I.).

H. GÖRGES—Researches on Alternating-Current Arc Lamps.—*E. T. Z.*, 1895, No. 34, p. 548 (I.).

J. PULUJ—Relation of the Phase Difference between the Primary Pressure Current to the Variations of the Load on the Secondary Network of a Transformer System.—*E. T. Z.*, 1895, No. 35, p. 557.

L. BRUNHES—The Electric Installations of Rotterdam.—*Ecl. El.*, vol. 4, No. 37, p. 489 (I.).

E. J. BRUNSWICK—The Klostermann Arc Lamp.—*Ibid.*, p. 502 (I.).

ANON.—Three-Phase Installation at Winooski.—*Ibid.*, p. 510.

G. RICHARD—Arc Lamps.—*Ecl. El.*, vol. 4, No. 38, p. 534 (I.).

ANON.—The Arons Mercury Arc Lamp.—*Ibid.*, p 558.

M. F. WHITE—Consumption of Coal in Central Stations.—*Ibid.*, p. 560.

ANON.—Electric Light and Power Station at Copenhagen Harbour.—*E. T. Z.*, 1895, No. 36, p. 573 (I.).

C. P. STEINMETZ—The Monocyclic System.—*Ibid.*, p. 586 (I.).

ANON.—Again the Monocyclic and Polycyclic Systems.—*Ibid.*, p. 587 (I.).

E. SCHULZ—Power Transmission and Lighting Installation of Messrs. Delden & Co. in Gronau.—*E. T. Z.*, 1895, No. 37, p. 599 (I.).

Prof. JACKSON—The Choice of Transformers.—*Ecl. El.*, vol. 5, No. 44, p. 215 (I.).

J. PIGG—Coupling Compound Machines in Parallel. -*Ibid.*, No. 45, p. 265.

ANON.—The Economy of Parallel Arc Lighting working at Constant Difference of Potential.—*Ecl. El.*, vol. 5, No. 47, p. 368.

F. GUILBERT—The Illumination of Holophane Globes.—*Ibid.*, No. 48, p. 392, No. 51, p. 529 (S. I.).

Anon.—Relations between Pressure, Electrical Resistance, and Friction in the Contact of Dynamo Brushes.—*Ecl. El.*, vol. 5, No. 48, p. 420.

Anon.—La Goule Transmission of Power.—*Ibid.*, No. 50, p. 515 (I.).

M. J. Laffargue—Electric Distribution in Paris.—*Bull. Soc. Int.*, vol. 12, No. 122, p. 403.

Anon.—The Back E.M.F. of the Electric Arc.—*Ecl. El.*, vol. 5, No. 45, p. 265.

F. Tischendörfer—Some Remarks on the Niagara Transmission of Power.—*E. T. Z.*, No. 41, p. 651.

D. Paisley—European Glow Lamps.—*Ibid.*, p. 658.

Dr. Rasch—On the Diminution of the Charges made for Electrical Energy.—*E. T. Z.*, No. 47, p. 739.

E. Lohr—The Middle Conductor in Three-Wire Systems.—*E. T. Z.*, No. 48, p. 768 (I.).

W. Klug—The Electric Light Installation of the Central Railway in Munich.—*E. T. Z.*, No. 49, p. 761 (I.).

Anon.—Report of the Commission appointed by Representatives of the Electrical Industry with reference to the Glow Lamp Question.—*E. T. Z.*, No. 49, p. 778 (I.).

Julius Henbach—The Calculation of Electrical Conductors.—*Ibid.*, No. 50, p. 785 (I.).

DYNAMO AND MOTOR DESIGN.

S. Hanappe—Study of a Two-Pole Diphase Generator.—*Ecl. El.*, vol. 3, No. 22, p. 385 (I.).

G. Kapp—Predetermination of the Fall of Potential in Transformers.—*Ibid.*, p. 421 (I.).

F. Guilbert—Synchronous Motors and M. Blondel's Theory.—*Ecl. El.*, vol. 3, No. 23, p. 436.

S. Hanappe—Some Calculations relating to a Shunt Dynamo of the Manchester Type.—*Ibid.*, No. 24, p. 481 (I.).

R. V. Picou—Alternators.—*Ibid.*, p. 506 (I.).

S. Hanappe—Study of the Magnetic Field of an Electric Motor of the Brown Type for Two-Phase Currents.—*Ecl. El.*, vol. 4, No. 27, p. 5, No. 28, p. 58 (I.).

O. Chrytraens—Reactions of the Armature in a Simple Alternator and a Polyphase Machine.—*Ecl. El.*, vol. 4, No. 28, p. 77.

S. Hanappe—Study of a Five-H.P. Two-Phase Electric Motor, Brown Type.—*Ecl. El.*, vol. 4, No. 29, p. 114, No. 30, p. 159.

E. Boistel—Dynamos.—*Ecl. El.*, vol. 4, No. 31, p. 211.

— Dolivo-Dobrowolsky—On the Fall of Potential in Alternators.—*Ibid.*, p. 225.

A. Blondel—Some General Properties of Magnetic Rotating Fields.—*Ecl. El.*, vol. 4, No 32, p. 241, No. 33, p. 308, No. 34, p. 358 (I).

A. Rothert—Contribution to the Theory of Synchronous Induction Motors—*Ecl. El.*, vol. 4, No. 32, p. 273.

E. J. Brunswick—Modification of Characteristics with the Air Space of Direct-Current Dynamos—*Ecl. El.*, vol. 4, No. 35, p. 411.

G. BENISCHKE—A New Asynchronous Motor for Alternate Currents.—*Ibid.*, p. 417.

— DUEZ—On a Comparison between Direct- and Polyphase-Current Electric Motors.—*C. R.*, vol. 121, No. 3, p. 160.

W. G. RHODES—A Theory of the Synchronous Motor.—*Phil. Mag.*, vol. 40, No. 242, p. 56, No. 243, p. 195.

L. BAUMGARDT—Experimental Determination of the Armature Field in Direct-Current Machines.—*E. T. Z.*, 1895, No. 23, p. 344.

H. CAHEN—On the Theory of Single-Phase Motors.—*E. T. Z.*, 1895, No. 30, p. 463.

C. P. FELDMANN—On the Influence of the Form of the E.M.F. Curve on the No-Load Losses in Transformers.—*E. T. Z.*, 1895, No. 31, p. 479 (I.).

G. ROESSLER—The Behaviour of Transformers under the Influence of Alternating Currents of different Frequencies.—*Ibid.*, p. 489 (I.).

D. KORDA—Armature Reaction in Polyphase Dynamos.—*E. T. Z.*, 1895, No. 32, p. 499.

H. BEHN-ESCHENBURG—On the Calculation of Electric Power Transmission with Alternate Currents.—*E. T. Z.*, 1895, No. 34, p. 535.

S. HANAPPE—Experimental Study of a Small Dynamo of 20–35 Watts Output.—*Ecl. El.*, vol. 4, No. 36, p. 433 (I.).

C. P STEINMETZ—Elementary Explanation of the Monocyclic System.—*Ecl. El.*, vol. 4, No. 38, p. 556 (I.).

E. DANIELSON—Graphic Theory for the Design of Polyphase Inductor Motors.—*E. T. Z.*, 1895, No. 37, p. 601.

F. A. WESSEL—Shunt Motor with Variable Speed.—*E. T. Z.*, 1895, No. 39, p. 625 (I.).

A. BLONDEL—Some Remarks on the Leakage Coefficient in Polyphase Motors.—*Ibid.*, p. 625.

P. BOUCHERST—Synchronous and Non-Synchronous Alternating-Current Motors.—*Ecl. El.*, vol. 5, No. 44, p. 193.

S. HANAPPE—Magnetic Leakage of Dynamos.—*Ecl. El.*, vol. 5, No. 49, p. 434 (S. I.).

A. BLONDEL—The Effect of Magnetic Leakage in Rotatory Field Motors.—*Ecl. El.*, vol. 5, No. 49, p. 442, No. 51, p. 540 (S. I.).

E. BOISTEL—Dynamos, Transformers, &c.—*Ecl. El.*, vol. 5, No. 44, p. 247.

A. BLONDEL—Magnetic Leakage in Rotary Field Motors.—*Ecl. El.*, vol. 5, No. 45, p. 253, No. 46, p. 296 (S. I.).

WALDEMAR MEISSNER—On the Electrical Design of Rock-Boring Machines, and the Rock-Boring System of Siemens & Halske.—*E. T. Z.*, No. 40, p. 641 (S. I.).

EMIL KOLBEN—Some Characteristic Qualities of Synchronous Motors.—*E. T. Z.*, No. 51, p. 802 (I.).

ELECTRIC TRACTION.

C. JACQUIN—The Sectional-Rail Electric Tramway System of Messrs. Claret & Vuilleumier.—*Ecl. El.*, vol. 3, No. 23, p. 448, No. 24, p. 449 (I.).

G. Pellissier—Electric Rail- and Tram-Ways.—*Ecl. El*, vol. 4, No. 28, p. 49, No. 30, p. 145, No. 35, p. 391, No. 36, p. 446 (S. I.).

Anon.—Triphase Currents in Electric Traction.—*Ibid.*, No. 29, p. 130 (I.).

G. Pellissier—The Metropolitan Railway of Chicago.—*Ecl. El.*, vol. 4, No. 32. p. 254 (I.).

Anon.—The Bersey Electric Carriage.—*Ibid.*, p. 268 (I.).

J. Reyval—Electric Traction on the Railway from Baltimore to Ohio.—*Ecl. El.*, vol. 4, No. 33, p. 299 (I.).

Anon.—Perturbation of Electric Instruments by Electric Traction.—*Ibid.*, p. 314.

Anon.—Henry's Method of Localisation of Faults in Electric Railways.—*Ecl. El.*, vol. 4, No. 35, p. 413.

Anon.—Cost of running Electric Tramways.—*E. T. Z.*, 1895, No. 23, p. 354.

Anon.—Electric Tramways in Berlin.—*E. T. Z.*, 1895, No. 25, p. 383, No. 27, p. 415, No. 35, p. 565.

Anon.—Electric Tramways in Munich.—*Ibid.*, No 27, p. 415.

— Nebricht and Others—Discussion on the Question of Disturbance of Scientific Institutions by Electric Railways.—*Ibid.*, p. 417, No. 28, p. 448.

Anon.—Electric Traction in Stuttgardt.—*E. T. Z.*, 1895, No. 30, p. 468.

Anon.—The Electric Metropolitan West Side Elevated Railway in Chicago.—*E. T. Z.*, 1895, No. 33, p. 519.

O. Bühring—Electric Tramway and Electric Light Station in Baden, near Vienna.—*E. T. Z.*, 1895, No. 34, p. 525 (I.).

A. Teirich—The Electric Railway Signal.—*Ibid.*, p. 531 (I.).

Anon.—Tramway with Underground Trolley.—*Ecl. El.*, vol. 4, No. 37, p. 508 (I.).

Anon.—The Hoerde Underground Conduit for Electric Tramways.—*Ecl. El*, vol. 4, No. 39, p. 601 (I.)

Anon.—The Diatto System of Electric Traction with Underground Conductor.—*Ibid.*, p. 602 (I.).

Anon.—Electric Tramways in Berlin.—*E. T. Z.*, 1895, No. 37, p. 607, No. 39. p. 630.

Anon.—Electric Tramways in Pressburg.—*Ibid.*, p. 607.

Henri Maréchal—Electric Tramways.—*Ecl. El.*, vol. 5, No. 44, p. 215.

G. H. Knox—Starting Electric Cars.—*Ibid.*, No. 44, p. 223.

G. Pellissier—Competition Scheme for the Mechanical Traction of Tramways in Paris.—*Ecl. El.*, vol. 5, No. 45, p. 241.

G. Pellissier—The Mechanical Traction of Tramways.—*Ecl. El.*, vol. 5, No. 46, p 289, No. 47, p. 345, No. 48, p. 400, No. 49, p. 447, No. 50, p. 503, No. 51, p. 545 (S.).

Anon.—King's Electric Carriage.—*Ecl. El.*, vol. 5, No. 46, p. 323 (I.).

Anon.—The Cattori Electric Railway.—*Ibid.*, No. 47, p. 369.

Anon.—Rail Return in Electric Traction.—*Ibid.*, No. 49, p. 464.

Anon.—The Progress of the Electro-Motor for Tramways.—*Ibid.*, No. 49, p. 466.

Siemens & Halske—Controlling Gear for Motors and Brakes for Electric Railways.—*Ecl. El.*, vol. 5, No. 50, p. 519.

Anon.—Hutin & Leblanc Electric Tramway.—*Ecl. El.*, vol. 5, No. 46, p. 319 (I.).

ANON —Electric Traction in Hamburg.—*E. T. Z.*, No. 40, p. 637 (I.).

ANON.—An Electric Railway with the Diatto Underground System of Conductors. —*E. T. Z.*, No. 43, p. 680 (I.).

ANON.—The Gesundbrunnen-Pankow Electric Street Tramway.—*E. T. Z.*, No. 44, p. 687 (I.).

MAGNETISM.

P. CURIE—Magnetic Properties of Bodies at different Temperatures.—*Jour. de Phys.*, June, 1895, p. 263.

E. VAN AUBEL—On the Hall Phenomenon and the Measurement of Magnetic Fields.—*Ecl. El.*, vol. 3, No. 22, p. 398 (I.).

ANON.—Secular Variation of Terrestrial Magnetism.—*Ibid.*, p. 420.

L. A. BAUER—On the Secular Movement of a Free Magnetic Needle.—*Ecl. El.*, vol. 3, No. 25, p. 571.

W. S. FRANKLIN—A New Method of Determining the Magnetic Properties of Iron.—*Ibid.*, p. 571.

A. DE TILLO—Law of Distribution of Mean Magnetism on the Surface of the Globe.—*C. R.*, vol. 121, No. 2, p. 97.

— MAURAIN—Vibrations of a Tuning Fork in a Magnetic Field.—*C. R.*, vol. 121, No. 5, p. 248.

L. BLEEKRODE—Note on the Magnetism of Asbestos.—*W. A.*, vol. 55, No. 6, p 398.

F. KOLÁČEK—On the Axial Character of Magnetic Lines of Force: A Conclusion derived from the Existence of the Hall Phenomenon.—*Ibid.*, No. 6, p. 503.

F. BEDELL—On Magnetic Potential.—*Beibl.*, vol. 19, No. 6, p. 513.

J. ZUCHRISTIAN—Experimental Demonstration of Magnetic Fields.—*Ibid.*, p. 515.

F. WOLFF—On the Influence of Magnetism on Chemical Reactions.—*Ibid*, No. 7, p. 581.

E. VAN AUBEL—On the Hall Phenomenon, and on the Measurement of Magnetic Fields.—*Beibl.*, vol. 19, No. 8, p. 659.

M. ASCOLI—On the Distribution of Induced Magnetism.—*Ibid.*, p. 659.

C. E. GUYE—On Induction in Armoured Cables.—*Ibid.*, p. 661.

A. SCHMIDT—Michael Faraday's View of the Earth's Magnetism.—*Ibid.*, p. 667.

F. W. BOWDEN—An Electro-magnetic Effect.—*Phil. Mag.*, vol. 40, No. 243, p 200.

C. L. WEBER—On Unipolar Induction.—*E. T. Z.*, 1895, No. 33, p. 513.

— MAURAIN—Variations of a Tuning Fork in a Magnetic Field.—*Ecl. El.*, vol 4, No. 36, p. 452 (I.).

ANON.—Magnetic Qualities of Forged Steel.—*Ecl. El.*, vol. 4, No. 38, p. 553.

R. A. FESSENDEN—On the Relation of Maximum Magnetic Induction to the Residual Magnetism.—*Ibid.*, p. 562.

A. SPIZYN—A New Method of Demonstration of the Magnetic Field.—*Beibl.*, vol. 19, No. 9, p. 709.

A. ABT—Residual Magnetism in Steel Tensile Test Pieces.—*Ibid.*, p. 709.

B. ROSING—Alteration of Length of Iron Wire in Magnetisation.—*Ibid.*, p. 710.

W. PEUKERT—On the Transmission of Magnetisation in Iron.—*E. T. Z.*, 1895, No. 38, p. 611.

C. P. Steinmetz—The Law of Hysteresis: Part III.—The Theory of Ironclad Inductive Resistances.—*E. T. Z.*, 1895, No. 39, p. 623 (I.).

Wladimir de Nikolaieve—Experiments to Demonstrate the Action of a Current on a Magnetic Pole.—*Bull. Soc. Int.*, vol. 12, No. 122, p. 472.

— De Bernadieres—The Construction of New Magnetic Charts.—*C. R.*, vol. 121, No. 20, p. 679; *Ecl. El.*, vol. 5, No. 48, p. 385.

I. Klemencic—The Circular Magnetisation of Iron Wires.—*W. A.*, No. 11, p. 574 (I.).

P. Culmann—The Magnetisation of Iron in Weak Fields.—*W. A.*, No. 11, p. 602.

Professor K. Zickler—The Magnetisation of Iron.—*E. T. Z.*, No. 51, p. 806 (I.).

INSTRUMENTS AND MEASUREMENTS.

A. Abraham and J. Lemoine—On the Measurement of very High Potentials.—*Ecl. El.*, vol. 3, No. 23, p. 433 (I.); *Jour. de Phys*, vol. 4, October, p. 466 (I.).

R. Arnoux—The Chauvin & Arnoux Volt and Ammeters.—*Ecl. El.*, vol. 3, No. 25, p. 566 (I.).

H. Leitch and A. Shaw—Measurements of the Capacity of Electrolytic Condensers.—*Ibid.*, p. 568.

Anon.—Life and Efficiency Curves of Incandescence Lamps.—*Ecl. El.*, vol. 3, No. 26, p. 596.

Anon.—Hartnell Automatic Rheostats.—*Ecl. El.*, vol. 4, No. 28, p. 73 (I.).

Anon.—Hunt & Meredith Indicator and Tachometer.—*Ecl. El.*, vol. 4, No. 29, p. 131 (I.).

Anon.—The Carbonelle Microphone.—*Ibid.*, No. 30, p. 170 (I.).

O. Froelich—Insulation Indicator for Electric Systems of Mains when Working.—*Ibid.*, p. 176.

W. Tchikoleff—The Use of Recording Instruments in Electric Work.—*Ecl. El.*, vol. 4, No. 31, p. 199, No. 32, p. 261 (I.).

H. C. Parker—On the Limits of Exactitude and the Actual Degree of Precision of Measurements of Electric Resistance.—*Ecl. El.*, vol. 4, No. 32, p. 284.

A. Broca—On the Elimination of Electro-motive Forces in the Measurement of Resistances.—*Ecl. El.*, vol. 4, No. 33, p. 291.

A. Wilke—Arrangement for Determining the Mechanical Phase Difference of Two Machines.—*Ecl. El.*, vol. 4, No. 34, p. 367 (I.).

J. Guinchant—The Conductivity of certain Ethers.—*C. R.*, vol. 120, No. 22, p. 1220.

C. Limb—On the Electro-motive Forces of the Clark, Gouy, and Daniell Standard Cells.—*C. R.*, vol. 121, No. 4, p. 199.

F. Koláček—The Mechanical Effects of a Varying Magnetic Field on a Closed Conductor, and their Utilisation for the Determination of Coefficients of Self-Induction.—*W. A.*, vol. 55, No. 8, p. 604.

A. Raps—A New Compensating Apparatus.—*W. A.*, vol. 55, No. 8, p. 737; *E. T. Z.*, 1895, No. 32, p. 507.

Grimaldi and Platania—On the Electrical Resistance of Metals in different Dielectrics: Part I.—Researches on the Resistance of Copper in Petroleum.—*Beibl.*, vol. 19, No. 6, p. 509.

B. O. PEIRCE—On the Electric Resistance of certain Bad Conductors.—*Ibid.*, p. 510.

E. F. HERROUN—On the Use of an Iodine Voltameter for the Measurement of Small Currents.—*Phil. Mag.*, vol. 40, No. 242, p. 91.

W. THIERMANN—An Apparatus for Exact Measurement of Pressure, Current, and Resistance.—*E. T. Z.*, 1895, No. 26, p. 387 (I.).

ANON.—Nissl's Double Microphone.—*E. T. Z.*, 1895, No. 33, p. 518 (I.).

G. HUMMEL—On Motor Meters.—*E. T. Z.*, 1895, No. 33, p. 522.

P. WEISS—On the Employment of the Ballistic Galvanometer in Cases where the Blow is not absolutely Instantaneous.—*Jour. de Phys.*, 1895, Sept., p. 420.

G. MEYER—Capillary Electrometer and Drop Electrodes.—*Ibid.*, p. 425.

ANON.—The Siemens & Halske Automatic Cut-Out.—*Ecl. El.*, vol. 4, No. 37, p. 505.

A. W. PORTER and D. K. MORRIS—The Measurement of Varying Currents in Inductive Circuits.—*Phil. Mag.*, vol. 40, No. 244, p. 256 (I.).

J. DEWAR and J. FLEMING—The Variation in the Electrical Resistance of Bismuth when cooled to the Temperature of Solid Air.—*Ibid.*, p. 303 (I.).

H. REUBENS—A Vibration Galvanometer.—*W. A.*, vol. 56, No. 9, p. 27.

W. EINTHOVEN—The Influence of the Resistance on the Rapidity of Movement of the Mercury in the Lippmann Electrometer.—*Beibl.*, vol. 19, No. 9, p. 708.

K. ZICKLER—Universal Electro-Dynamometer.—*Ibid.*, p. 708.

H. WILDE—On a Magnetometer which shows the Influence of Temperature on the Magnetisation of Iron and other Magnetic Substances.—*Ibid.*, p. 711.

J. BLONDIN—On the Absolute Measurement of Resistances.—*Ecl. El.*, vol. 5, No. 44, p. 208 (I.).

J. CUMA—Measurement of Resistances for High-Frequency Alternating Currents. —*Ibid.*, p. 230.

ANON.—The Marechal-Rigollet-Ducretet Relay.—*Ecl. El.*, vol. 5, No. 45, p. 263.

ANON.—The Clark & Wetherall Aperiodic Galvanometer.—*Ecl. El.*, vol. 5, No. 46, p. 319.

WLADIMIR DE NIKOLAIEVE.—On the Fall of Potential in a Rhumkorff Coil.—*Bull. Soc. Int.*, vol. 12, p. 403, p. 471.

VICTOR BIERNACKI—The Resistance of the Electric Spark.—*Ibid.*, p. 474.

H. PELLAT—Apparatus for Measuring the Specific Inductive Coefficients of Solid and Liquid Bodies.—*Jour. de Phys.*, vol. 4, Nov., p. 501.

S. W. HOLMAN—Galvanometer Design: Waste Space near the Needle.—*Phil. Mag.*, Dec., No. 247, p. 494 (I.).

T. MIZERNO—Note on Tinfoil Grating as a Detector for Electric Waves.—*Ibid.*, p. 497.

L. HOLBORN and W. WIEN—The Measurement of High Temperatures.—*W. A.*, No. 10, vol. 56, p. 360 (I.).

F. LINDE—Measurement of Dielectric Constants of Liquid Gases and the Mossotti-Clausius Formula.—*E. T. Z.*, No. 52, p. 820.

Dr. CLASSEN—On the Sensitiveness obtained with the Deprez Galvanometer.—*E. T. Z.*, No. 43, p. 676.

Dr. Theodor Bruger—On Motor Meters, with special reference to the Hartmann & Braun Alternating-Current Motor Meters.—*E. T. Z.*, No. 43, p. 677.

R. M. Friese—A Hot Wire Reflecting Instrument.—*E. T. Z.*, No. 46, p. 726.

F. Kohlrausch—On the Practical Use of the Kirchoff Wheatstone Bridge.—*E. T. Z.*, No 51, p. 807; *W. A.*, No. 10, vol. 56, part 2, p. 177.

Desiré Korda—A Method for Determining the Efficiency of Transformers.—*E. T. Z.*, No. 52, p. 813.

TELEGRAPHY AND TELEPHONY.

E. R. Collette—Long-Distance Telephony.—*Jour. Tel.*, vol. 19, No. 6, p. 121 (S. I.).

Anon.—Telegraphs and Telephones in Belgium in the Year 1893.—*Ibid.*, p. 126.

Anon.—Telegraphs in the Dutch Indies in the Year 1893.—*Ibid.*, p. 131.

Anon.—Telegraphs and Telephones in Austria in 1893.—*Ibid.*, p. 135.

F. Nissl—Automatic Switch for Telephone Circuits.—*Jour. Tel.*, vol. 19, No. 7, p. 145.

Anon.—Statistics of Telephony in 1893.—*Ibid.*, p. 149.

Anon.—Telegraphs and Telephones in Switzerland in 1894.—*Ibid.*, p. 157, No. 8, p. 176.

Anon.—Telegraphs and Telephones in Cochin-China and Cambodia in 1894.—*Ibid.*, p. 161.

A. Tobler—Improvements in Telephonic Apparatus.—*Jour. Tel.*, vol. 19, No. 8, p. 169, No 9, p. 193.

Anon.—Instruments and Tools for Use in Setting up Overhead Electric Conductors.—*Ibid.*, p. 181.

Anon.—Muirhead & Green's Telephonic Transmission.—*Ecl. El.*, vol. 3, No. 24, p. 511 (I.).

Anon.—Telephone of the Telephone Company of Berlin.—*Ecl. El.*, vol. 4, No. 32, p. 266 (I.).

J. H. West—Quadruplex Telegraphy in America.—*E. T. Z.*, 1895, No. 26, p. 395; *Ecl. El.*, vol. 4, No. 34, p. 369 (I.).

J. H. West—Telephone System in Stuttgart.—*E. T. Z.*, 1895, No. 23, p. 338, No. 24, p. 364, No. 25, p. 375, No. 30, p. 461 (I.).

Breisig and Bokelmann—Researches on the Form taken by the Current in Cables worked by Hughes Apparatus.—*E. T. Z.*, 1895, No. 26, p. 392, No. 27, p. 409 (S. I.).

Anon.—Telephony in Japan.—*E. T. Z.*, 1895, No. 27, p. 414.

Anon.—Callender's Automatic Central Switch for Telephone Exchanges.—*E. T. Z.*, 1895, No. 33, p. 516.

A. Kruit—Telephony in the Netherlands.—*Jour. Tel.*, vol. 19, No. 9, p. 195, No. 10, p. 218 (S.).

J. H. West—Quadruplex Telegraphy in America.—*Ibid.*, p. 200.

Anon.—The Pacific Ocean Cable.—*Ibid.*, p. 204, No. 10, p. 230 (S.).

Anon.—Mechanical Representation of the Working of Submarine Cables.—*Ecl. El.*, vol. 4, No. 36, p. 464.

ANON.—Electrical Communication between the Land and Lightships.—*Ecl. El.*, vol. 4, No. 39, p. 600 (I.).

P. BACHMETJEW—On Electric Earth Currents.—*Beibl.*, vol. 19, No. 9, p. 715.

A. E. COLLETTE—Long-Distance Telephony.—*E. T. Z.*, 1895, No. 37, p. 604, No. 38, p. 615, No. 39, p. 627, No. 40, p. 644 (S. I.).

A. HESS—The Telephonic Systems of M. Claudio Baradat.—*Ecl. El.*, vol. 5, No. 44, p. 198 (I.).

ANON.—The Jelany Perforated Tape System of Telegraphy.—*Ibid.*, p. 213 (I.).

ANON.—The Higgin Telephone.—*Lum. El.*, vol. 5, No. 49, p. 464.

M. PIERARD—A Method for Preventing the Noise due to Induction from Electric Traction in Single-Line Telephones.—*Jour. Tel.*, vol. 19, No. 10, p. 218 (I.).

M. BAUDOT—Multiple Telegraphy.—*Jour. Tel.*, vol. 19, No. 11, p. 241, No. 12, p. 265 (S. I.).

ANON.—Comparative Telegraph Statistics of 1894.—*Ibid.*, vol. 19, No. 11, p. 247.

ANON.—Telegraphy in Great Britain since 1870.—*Ibid.*, p. 254.

ANON.—The Telegraph and Telephone in Great Britain, 1894–95.—*Ibid.*, No. 12, p. 275.

G. H. LINCKEUS—Telegraphy in the Netherlands during 1894.—*Ibid.*, No. 12, p. 274.

A. HULTMAN—On the Size of a Telephone Exchange.—*E. T. Z.*, No. 46, p. 729.

W. SCHRADER—The Use of Telephone Alarums on Telegraph Circuits.—*E. T. Z.*, No. 47, p. 738 (I.).

ANON—Hall's Self-Acting Automatic Railway Signal.—*E. T. Z.*, No. 48, p. 754 (I.).

ED. LANDRATH—The Development of the Telephone Service in Berlin.—*E. T. Z.*, No. 49, p. 774, No. 50, p. 787, No. 51, p. 805, No. 52, p. 817.

ELECTRO-CHEMISTRY.

G. GIN—Researches on the Electrolysis of Sugared Juices.—*Bull. Soc. Int.*, vol. 12, No. 119, p. 286.

ANON.—The Hermite-Paterson & Cooper Electrolyser for Domestic Use.—*Ecl. El.*, vol. 3, No. 22, p. 418 (I.).

ANON.—The Shoenmehl-Platt Battery.—*Ibid.*, p. 419 (I.).

— MAJORANA—Formation of Cuprite in the Electrolysis of Copper Sulphate.—*Ecl. El.*, vol. 3, No. 23, p. 466.

ANON.—Hargreaves & Bird Electrolyser.—*Ibid.*, No. 24, p. 512 (I.).

ANON.—The Coles & Walker System of Electric "Galvanisation."—*Ibid.*, p. 513 (I.).

A. RIGAUT—On the Subject of Calcium Carbide.—*Ecl. El.*, vol. 3, No. 25, p. 555.

ANON.—Cox Thermo-electric Battery.—*Ecl. El.*, vol. 4, No. 27, p. 26 (I.).

ANON.—The Vautin Process for Electrolytic Production of the Metals of the Alkalies.—*Ibid.*, p. 27 (I.).

ANON.—Kellner Electrodes.—*Ecl. El*, vol. 4, No. 28, p. 75 (I.).

ANON.—On Electrolysis by Alternate Currents.—*Ecl. El.*, vol. 4, No. 32, p. 269.

— MORISOT—On a Constant Battery of High Electro-motive Force.—*Ecl. El.*, vol. 4, No. 33, p. 297.

— JUNIUS—On the Efficiency of Junius Batteries.—*Ibid.*, p. 297.

— HURTER—The Efficiency of Electrolytic Apparatus.—*Ecl. El.*, vol. 4, No. 35, p. 415.

H. DESLANDRES—Spectroscopic Study of the Carbons of an Electric Furnace.—*C. R.*, vol. 120, No. 23, p. 1259.

— MORISOT—On a New Primary Cell.—*C. R.*, vol. 121, No. 5, p. 251.

J. E. MYERS—On the Influence of Dissolved Salts on the Silver Voltameter.—*W. A.*, vol. 55, No. 6, p. 288.

E. VOGEL—On the Galvanic Polarisation of Nickel, Cobalt, and Iron.—*W. A.*, vol. 55, No. 8, p 610.

— HÄUSSERMANN—Direct Utilisation of Chemical Energy of Gases for the Production of Electric Currents.—*Beibl.*, vol 19, No. 6, p. 511.

E. FRANKE—The Electric Conductivity of Salts and Acids in Aqueous Solutions.—*Beibl*, vol. 19, No 7. p. 574.

B. PIESCH—Alteration of the Electrical Resistance of Aqueous Solutions and of Galvanic Polarisation with Pressure.—*Ibid.*, p. 575.

A. CAMPETTI—On the Speed of Ions.—*Beibl.*, vol. 19, No. 8, p. 656.

F. RICHARZ—On Helmholtz's Electro-chemical Theory, and some Conclusions deduced from the same.—*Phil. Mag.*, vol. 39, No. 241, p. 529.

W. PEUKERT—On Electrolysis with Alternate Currents.—*E. T. Z.*, 1895, No 23, p. 345 (I.).

M. VON DOLIVO-DOBROWOLSKY—On Phase Displacement of Alternating Currents by Electrolysis.—*E. T. Z*, 1895, No. 25, p. 381.

ANON.—Electricity direct from Carbon.—*E. T. Z.*, 1895, No. 32, p. 502.

O SCHMIDT—On the Production of Electricity by Chemical and Thermo-chemical Means.—*E. T. Z*, 1895, No. 35, p. 569.

J. REYVAL—The Transformation of Carbon into Graphite: The Girard & Street Furnaces.—*Ecl. El.*, vol. 4, No. 36, p. 454.

ANON.—The Solvay Process of Separation of Caustic Alkalies from Liquors obtained by Electrolysis of Alkaline Salts.—*Ibid.*, p. 461.

ANON.—The Waite Electrolyser.—*Ibid.*, p. 461 (I.).

ANON.—The Gautier Electrolyser.—*Ibid.*, p. 462 (I.).

P. JANET—Electro-Chemistry: The General Laws of Electrolysis.—*Ecl. El.*, vol. 4, No. 37, p. 481.

P. JANET—Electro-Chemistry. The Movement of Ions.—*Ecl. El.*, vol. 4, No. 39, p. 577.

J. M. LOVEN—The Electrical Conductivity and Affinity of the Manganese Salts.—*Beibl.*, vol., 19. No. 9, p. 702.

A. DITTE—Phenomena occurring in the Leclanché and Allied Cells.—*Ibid.*, p. 703.

A. H. BUCHERER—The Production of Electricity direct from Coal.—*Beibl.*, vol. 19, No. 9, p. 704.

W. GREEFF—On the Internal Resistance of Galvanic and other Cells.—*Ibid.*, p. 706.

A. H. BUCHERER—On the Nature of Electrolysis.—*Ibid.*, p. 707.

ANON.—The Electric Production of Titanium and Carbide of Titanium.—*Ecl. El.*, vol. 5, No. 45, p. 262.

ANON.—The Electric Manufacture of Zinc by the Siemens & Halske Process.—*Ibid.*, p. 262 (I.).

ANON.—Maclean & Claybull Gold Electrolyser.—*Ecl. El.*, vol. 5, No. 46, p. 320.

G. FARMER—The Application of Electro-Metallurgy to the Refining of Silver, and incidentally of other Metals.—*Ecl. El.*, vol. 5, No. 47, p. 363.

ANON.—The Pelatan System for the Electrolytic Separation of Precious Metals.—*Ecl. El.*, p. 416, No. 48, vol. 5 (I).

ANON.—The Solvay Centrifugal Electrolyser.—*Ibid.*, p. 417 (I.).

ANON.—The Kellner Mercury Electrolyser.—*Ibid.*, p. 417 (I.).

ANON.—The Stornier Mercury Electrolyser.—*Ibid.*, p. 417 (I.).

M. G. DELVALEZ—Parasitical Electrodes.—*C. R.*, No. 15, vol. 121, p. 492

M. CODY—The Electro-capillary Properties of Dilute Sulphuric Acid.—*C. R.*, vol. 121, No. 22, p. 765.

HENRI MOISSAN—On the Presence of Sodium in Electrolytically obtained Aluminium.—*C. R.*, No. 23, vol. 121, p. 794.

R. LANG—On the Relation between Dielectric Constants and the Chemical Values —*W. A.*, No. 11, p. 534.

ACCUMULATORS.

ANON.—The Griffin Accumulator (1895).—*Ecl. El.*, vol. 3, No. 22, p. 417 (I.).

ANON.—The Hammacher (1894) Accumulator.—*Ecl. El.*, vol. 4, No. 30, p. 172.

G. MARGAINE—Critical Study of the Lead Accumulator; Description of the Blot Accumulator.—*Ecl. El.*, vol. 4, No. 33, p. 290.

ANON.—The Epstein Accumulator.—*Ecl. El.*, vol. 5, No. 49, p. 463.

ANON.—Improvements in Accumulators.—*Ibid.*, No. 51. p. 545.

ANON.—The Schanschieff Accumulator.—*Ibid.*, vol. 5, No. 46, p. 316.

K. STRECKER and TH. KARRASS—Measurement of Capacity of a Storage Battery after Five Years' Work.—*E. T. Z.*, No. 42, p. 669 (I.).

STATIC ELECTRICITY.

E. BRANLY—The Employment of Tubes filled with Particles of Matter in the Study of Interference Phenomena.—*Jour. de Phys.*, June, 1895, p. 273.

G. GOURÉ DE VILLEMONTÉE—On the Determination of the Electrical Constants of Water.—*Ibid.*, July, 1895, p. 293.

A. PEROT—The Measurement of Sparking Distances in the Hertz Experiments by means of an Unsymmetrical Micrometer.—*Ecl. El.*, vol. 3, No. 22, p. 414 (I.).

A. RIGHI—Production of Long Sparks on the Surface of Water.—*Ecl. El.*, vol. 4, No. 27, p 21 (I.).

— MAGOTTO—On the Dielectric Constant of certain Substances, and in particular of Glass.—*Ibid.*, p. 33.

— DUHEM—On the Theoretical Interpretation of the Hertz Experiments.—*Ecl. El.*, vol. 4, No. 33, p. 295.

A. BROCA—Experimental Study of the Disruptive Spark.—*Ecl. El*, vol. 4, No. 34, p. 343, No. 35, p. 401 (I.).

P. V. Schaffers—On the Theory of the Wimshurst Machine.—*Ecl. El.*, vol. 4, No. 35, p. 421 (I.).

— Gouy—On the Apparent Attractions and Repulsions of Electrified Conductors in a Fluid Dielectric.—*C. R.*, vol. 121, No. 1, p. 53.

H. Bordier—New Method of Measurement of Electric Capacity, based on the Sensibility of the Skin.—*Ibid.*, p. 56.

J. E. Myers—On the Faraday Law in connection with Currents of Frictional Electricity.—*W. A.*, vol. 55, No. 6, p. 297.

W. Lietzau—The Capacity of Mercury for Polarisation.—*Ibid.*, p. 338.

O. Lehmann—On the Glow and Striation of the Arc Light, and of Discharges in Rarefied Gases.—*Ibid.*, p. 361.

J. v. Geitler—Oscillation in Complicated Exciters of Hertzian Waves.—*W. A.*, vol. 55, No. 7, p. 513.

V. Biernacki—A Simple Objective Representation of the Hertzian Mirror Experiments.—*Ibid.*, No. 8, p. 599 (I.).

A. Oberbeck—On the Course of the Electric Oscillations in the Tesla Experiments.—*W. A.*, vol. 55, No. 8, p. 623 (I.).

P. Drude—A Convenient Method of Demonstration of the Indices of Electric Refraction of Liquids.—*Ibid.*, p. 633 (I.).

G. Jaumann—Inconstancy of Sparking Pressure.—*Ibid.*, p. 656 (I.).

G. Giazzi—On the Use of an Electrometer with a Needle which receives its Charge by means of a Thread dipping in a Liquid.—*Beibl.*, vol 19, No. 6, p. 507.

G. Vincentini and M. Cinelli—Passage of Electricity through Gases which surround a Wire made Red-Hot by an Electric Current.—*Ibid.*, p. 521.

J. Trowbridge and M. Duane—Speed of Electric Waves.—*Phil. Mag.*, vol. 40, No. 243, p. 241; *Beibl.*, vol. 19, No. 7, p. 582.

F. E. Nipher—On Electrical Capacity and the Energy of an Electric Charge.—*Beibl.*, vol. 19, No. 8, p. 643.

D. Mazotto—On the Dielectric Constant of certain Substances, and especially of Glass.—*Ibid.*, p 645.

F. Pockels—On the Influence of the Electrostatic Field on the Optical Behaviour of Piezo-electric Crystals.—*Ibid.*, p. 650.

F. Bohnert—Electrostatics: An Attempt to Demonstrate in an Elementary Manner their Chief Laws.—*Ibid.*, p. 663.

P. Lebedew—On Double Refraction of the Rays of Electric Energy.—*W. A.*, vol. 56, No. 9, p. 1.

Schmidt and Rühlmann—The Holtz Influence Machine.—*Ibid.*, p. 167.

F. Busch—A New Electrical Machine.—*Beibl.*, vol. 19, No. 9, p. 699.

Sheldon, Leitch, and Shaw.—On the Capacity of Electrolytic Condensers.—*Ibid.*, p. 701.

M. Lamotte—M. O. Lehmann's Researches on Electric Charges in Gases.—*Ecl. El*, vol. 5, No. 47, p. 361 (I).

M. Bordier—Experimental Researches on the Electric "Wind."—*C. R.*, No. 24, vol. 121, p. 907.

M. H. Pellat—Measure of the Force acting on a Non-Electrified Dielectric placed in an Electric Field.—*C. R.*, No. 25, p. 938.

K. E. F. Schmidt and Hans Rühlmann—The Holtz Influence Machine —*E. T. Z.*, No. 45, p. 717.

ATMOSPHERIC ELECTRICITY.

N. Piltschikoff—New Photographs of Lightning.—*C. R.*, vol. 121, No. 5, p. 250.

E. Kircher—Measurements of Atmospheric Potential in Absolute Units.—*Beibl.*, vol. 19, No. 9, p. 714.

THEORY.

W. de Nikolaieve—Two Methods of Discovering and Studying Currents in Open Metallic Circuits, and Displacement Currents in Dielectrics.—*Jour. de Phys.*, June, 1895, p. 245.

W. de Nikolaieve—Method of Determination of the Coefficient of Self-Induction in a Coil.—*Ibid.*, August, 1895, p. 364.

F. Bedell and A. Crehore—Resonance of Transformer Circuits.—*Ecl. El.*, vol. 3, No. 26, p. 577 (L).

G. Saguac—On Resistance to a Variable Current.—*Ecl. El.*, vol. 4, No. 28, p. 66.

B. Brunhes—The Electro-magnetic Theory of Light and Crystalline Absorption. —*Ecl. El.*, vol. 4, No. 31, p. 193, No. 34, p. 352, No. 38, p. 529, No. 39, p. 596

E. Ketteler—Derivation of the Equations of the Electro-magnetic Theory of Light from the Experimental Facts of the Optics of Crystals.—*W. A.*, vol. 55, No. 7, p. 525.

G. Giorgi—Definition of Reactance.—*Beibl.*, vol. 19, No. 8, p. 662.

C. Heinke—The Laws of Electric Circuits.—*E. T. Z.*, 1895, No. 32, p. 509.

K. Schreber—On the Theory of the Capillary Electrometer.—*Jour. de Phys.*, September, 1895, p. 424.

P. Duhem—On the Theoretical Interpretation of the Hertz Experiments.—*Ecl. El.*, vol. 4, No. 37, p. 494.

C. V. Zenger—The Electro-dynamic Theory of the System of the Universe.—*Ecl. El.*, vol. 4, No. 38, p. 547.

G. Benischke—The Use of Condensers in Alternate-Current Circuits.—*E. T. Z.*, 1895, No. 38, p. 612 (I.).

H. Poincaré—Larmor's Theory.—*Ecl. El.*, vol. 5, No. 48, p. 385 (S.).

E. Raverot—On the Dimensions of Physical Quantities.—*Ecl. El.*, vol. 5, No. 50 p. 481.

J. J. Thomson—The Relation between the Atom and the Charge of Electricity carried by it.—*Phil. Mag.*, No. 427, December, p. 511 (I.).

H D. Hurmuzescu—A New Determination of the Ratio v between Electrostatic and Electro-magnetic Units.—*C. R.*, No. 23, vol. 121, p. 815.

M. D'Arsonval—On Imbert's Electro-capillary Theory of Muscular Contraction. —*C. R.*, No. 24, vol. 121, p. 906.

Proteus Steinmetz—The Law of Hysteresis (Part III.), and the Theory of Ironclad Inductive Resistances.—*E. T. Z.*, No. 41, p. 652, No. 42, p. 666 (S. I.).

Rob. M. Friese—On the Law of Hysteresis.—*E. T. Z.*, No. 42, p. 669.

Alex. Rothert—Contribution to the Theory of Rotary Field Motors.—*E. T. Z.*, No. 45, p. 705 (I.).

C. P Steinmetz—The Theory of Induction Motors.—*E. T. Z.*, No. 46, p. 727 (I.).

Hans Gorges—The Theory of Asynchronous Alternating-Current Motors.—*E. T. Z.*, No. 48, p. 750, No. 49, p 768, No. 50, p. 789, No. 51, p. 803 (I.).

VARIOUS.

A. Blondel—The Determination of the Mean Spherical Intensity of Sources of Light.—*Ecl. El.*, vol. 3, No. 22, p. 406, No. 25, p. 538, No. 26, p. 583 (S. I.).

Anon.—Siemens-Dieselhorst Cables.—*Ibid.*, p. 419.

Anon.—Temperature of the Filaments of Incandescent Lamps.—*Ibid*, p. 420.

G. Pellissier—The Invention of the Electro-Magnet.—*Ecl. El.*, vol. 3, No. 23, p. 472.

E. Boistel—Professor Thompson on a Neglected Experiment of Ampère's.—*Ecl. El.*, vol. 4, No. 27, p. 39 (I.).

E. Boistel—The Invention of the Electro-Magnet.—*Ecl. El.*, vol. 4, No. 29, p. 136.

Houston and Kennelly—Measurement of Illumination.—*Ecl. El.*, vol. 4, No. 30, p. 172.

A. Hess—The Question of Dielectric Hysteresis.—*Ecl. El.*, vol. 4, No. 31, p. 205.

Anon.—Light Railways in Belgium.—*Ibid.*, p. 215.

Anon.—Siemens & Halske Cut-Out.—*Ibid.*, No. 32, p. 266 (I.).

Anon.—Ferranti Cut-Out.—*Ibid.*, p. 267.

J. Elster and H. Geitel—On the Relation between the Photo-electric Current and the Azimuth and the Angle of Incidence of the Light.—*Ecl. El.*, vol. 4, No. 32, p. 278.

J. Blondin—The Bordeaux Meeting of the French Association for the Advancement of Science.—*Ecl. El.*, vol 4, No. 33, p. 289.

— Broca—On the Polarity of the Rhumkorff Coil.—*Ibid.*, p. 298.

Anon.—Felten & Guilleaume Low-Capacity Cable.—*Ibid.*, p. 313 (I.).

Anon.—Thomson Brush-Holder.—*Ibid.*, p. 313.

Anon.—American Vindication of Priority in Telegraphy without Wires.—*Ibid.*, p. 315.

G. Pellissier—Electricity at Bordeaux.—*Ecl. El.*, vol. 4, No. 34, p. 337.

Anon.—Electric Distribution of the Time at Toronto—Wright's System.—*Ibid.* p. 365 (I).

A. Hess.—New Harmonic Analysers.—*Ecl. El.*, vol. 4, No. 35, p. 385 (I.).

— D'Arsonval—Research on the Electric Discharge of the Torpedo.—*C. R.*, vol. 121, No. 3, p. 145.

— Marey—Observations on the above Paper.—*Ibid.*, p. 150.

G. T. Lhuillier—On the Conductivity of certain Mixtures of Metallic Filings and Dielectrics.—*C. R.*, vol. 121, No. 8, p. 345.

C. V. Zenger—The Electro-dynamic System of the Universe.—*C. R.*, vol. 121, No. 9, p. 386.

A. Righi—Double Refraction of Electric Rays.—*W. A.*, vol. 55, No. 6, p. 389.

E. Pringsheim—On the Conduction of Electricity through Hot Gases.—*W. A.*, vol. 55, No. 7, p. 507.

J. Elster and H. Geitel—Experiments on Light and Electricity made with Polarised Light.—*W. A.*, vol. 55, No. 8, p. 684 (I.).

W. Voigt—Piezo- and Pyro-Electricity, Dielectric Influence, &c.—*Ibid.*, p. 701.

F. Swarts—Some Remarks on Curves of Electrical Conductivity.—*Beibl*, vol. 19, No. 6, p. 509.

K. Domalip—On Harmonic Currents, Hysteresis, and Eddy-Currents.—*Ibid.*, p. 519.

P. Duhem—On the Pressure in Dielectric and Magnetic Media.—*Beibl.*, vol. 19, No. 7, p. 573.

P. Zeeman—Measurements of the Kerr Phenomenon with Normal Polar Reflection on Iron and Cobalt.—*Ibid.*, p. 579.

M. Cantor—On the Dispersal of Electricity by Light.—*Ibid.*, p. 583.

H. Moissan—Some New Forms of Electric Furnace with Movable Electrodes and Cover.—*Beibl.*, vol. 19, No. 8, p. 648.

C. E. Guillaume—An Experimental Demonstration of Thermo-electric Currents. —*Ibid.*, p. 649.

M. Rajna—On the Daily Excursion of the Magnetic Declination in Milan in its Relation to the Sun-Spot Period.—*Ibid.*, p. 666.

S. P. Thompson—Note on a Neglected Experiment of Ampère.—*Phil. Mag.*, vol. 39, No. 241, p. 534.

Dewar and Fleming—Thermo-electric Powers of Metals and Alloys between the Temperature of the Boiling Point of Water and the Boiling Point of Liquid Air.—*Phil. Mag.*, vol. 40, No. 242, p. 95 (I.).

Anon.—Gebrueder Naglo's Multiple Switch.—*E. T. Z.*, 1895, No. 23, p. 349 (I.).

Anon.—The New Works of the Westinghouse Company in America.—*E. T. Z.*, 1895, No. 30, p. 466 (I.).

Anon.—Metrical Screw Threads.—*Ibid.*, No. 34, p. 545.

W. Wedding—Comparative Measurements of different Sources of Light.—*Ibid.*, p. 554.

R. Busquet—Mnemonic Rules in Electricity.—*Ecl. El.*, vol. 4, No. 38, p. 550.

G. Pellissier—Electricity at Bordeaux.—*Ecl. El.*, vol. 4, No. 39, p. 588 (I.).

A. Casanova—The Malignant Process for obtaining a Vacuum in Incandescent Lamps.—*Ecl. El.*, vol. 4, No. 39, p. 602.

S. Bidwell—The Electrical Properties of Selenium.—*Phil. Mag.*, vol. 40, No. 244, p. 233.

R. Reiff—On the Movement of Electricity in Solutions and Metals.—*W. A.*, vol. 56, No. 9, p. 42.

Bachmetjew and Stamboljeff—On Electrical Currents arising through the Heating of Homogeneous Metal Wire.—*Beibl.*, vol. 19, No. 9, p. 710.

Adolphe Perrin—On Mechanical and Electrical Units.—*Ecl. El.*, vol. 5, No. 44, p. 201, No. 45, p. 260, No. 47, p. 355 (S.).

— Mettetal—An Electric Phenomenon.—*Ecl. El.*, vol. 5, No. 45, p. 279.

J. Reyval—The Electric Properties of Selenium.—*Ecl. El*, vol. 5, No. 46, p. 311.

ANON.—The Treatment of Persons who have received an Electric Shock.—*Ecl. El.*, vol. 5, No. 46, p. 316.

ANON.—The Brutschke and Schimpff System of Electric Ploughing.—*Ecl. El.*, vol. 5, No. 47, p. 367.

A. LEGOUX—A New Application of Electricity to Agriculture.—*Ibid.*, p. 374.

P. LEBEDEW—The Double Refraction of Electric Waves of Energy.—*Ecl. El.*, vol 5, No. 48, p. 425.

ANON.—The Holland Process of Tempering Wires.—*Ibid.*, p. 463 (I.).

G. RICHARD—The Mechanical Applications of Electricity.—*Ibid.*, No. 50, p. 488, No. 51, p. 536 (S. I.).

WLADIMIR DE NIKOLAIEVE—Explanation of the Repulsion of Elihu Thomson's Ring by the Reaction of Magnetic Lines of Force and Effects of Self-Induction.—*Jour. de Phys.*, vol. 4, Nov., p. 519.

E. VAN AUBEL—Relation between the Thermal and Electric Conductivities of Alloys.—*Jour. de Phys.*, vol. 4, Nov., p. 522 (I.).

F. BEAULARD—On the Specific Inductive Capacity of Glass.—*Ibid.*, p. 552 (I.).

CAMILLE DARESTE—Researches on the Influence of Electricity on the Evolution of a Chicken's Embryo.—*C. R.*, No. 25, p. 955.

E. WIEDERMANN and P. SCHMIDT—On the Phosphorescence of Solid Bodies and Solutions.—*W. A.*, vol 56, No. 10, p. 177.

P. LENARD—On the Absorption of Kathode Rays.—*W. A.*, No. 10, p. 255 (I.).

H. PAALZOZ and F. NIELSEN—The Passage of Electricity through Gases.—*Ibid.*, p. 276, No. 12, p. 700.

O. LEHMANN—Kathode Rays and the Continuous Discharge through Gases.—*W. A.*, vol. 56, No. 10, p. 304 (I.).

W. EINTHOVEN—Isolation from Surrounding Vibrations.—*E. T. Z.*, No. 45, p. 717.

J. ELSTER and H. GEITEL—Electric Experiments with Polarised Light.—*E. T. Z.*, No. 46, p. 731.

H. LUGGIN—Polarisation Phenomena in Metal Films.—*W. A.*, vol. 56, No. 10, p. 347.

H. HAGA—The Influence of Electric Waves on the Galvanic Resistance of Metallic Conductors.—*W. A.*, No. 11, p. 571.

C. CHRISTIANSER—Experimental Researches on the Origin of Galvanic Electricity.—*W. A.*, No. 12, p. 644 (I.).

G. MEYER—On the Difference of Potential between Metals and Liquids.—*W. A.*, No. 12, p. 680.

K. MACK—On the Double Refraction of Electric Waves.—*W. A.*, No. 12, p. 717.

J. ELSTER and H. GEITEL—Changeable Light Phenomena in Rarefied Gases produced by Electric Oscillations.—*W. A.*, No. 12, p. 733 (I.).

ANON.—Sigmund Schukert.—*E. T. Z.*, No. 40, p. 635 (I.).

Dr. EMIL LIEBENTHAL—The Effect of the Nature of the Surrounding Air on the Hefner Lamp and the Pentane Lamp.—*E. T. Z.*, No. 41, p. 655.

HUGO LANGNER—Siemens & Halske Controlling Apparatus for Lifts.—*E. T. Z.* No. 42, p. 663.

ANON.—The Inconstancy of Sparking Potentials.—*E. T. Z.*, No. 42, p. 670

O. LEHMANN—On the Stratification of Electric Discharges in Rarefied Gases.—*E. T. Z.*, No. 42, p. 670.

Dr. G. ROESSLER—The Graphic Representation of Changes in Alternating-Current Curves.—*E. T. Z.*, No. 43, p. 681, No. 45, p. 708 (I.).

ANON.—Electric Time Synchronising in the United States.—*E. T. Z.*, No. 44, p. 691, No. 45, p. 710 (I.).

ANON.—The Isar Works.—*E. T. Z.*, No. 45, p. 700 (I.).

J. TEICHMÜLLER—The Electric Exhibition at Karlsruhe.—*E. T. Z.*, No. 45, p 703 (I.).

P. LEHEDEZD—The Refraction of Electric Rays.—*E. T. Z.*, No. 45, p. 717.

W. H. JULIUS—A Method of Protecting Measuring Instruments against Terrestrial Vibrations.—*E. T. Z.*, No. 45, p. 717.

NOTICE.

1. The Institution's Library is open to members of all Scientific Bodies, and (on application to the Secretary) to the Public generally.

2. The Library is open (except from the 14th August to the 16th September) daily between the hours of 11.0 a.m. and 8.0 p.m., except on Thursdays, and on Saturdays, when it closes at 2.0 p.m.

An Index, compiled by the late Librarian, to the first ten volumes of the Journal (years 1872–81), and an Index, compiled under the direction of the Secretary, to the second ten volumes (years 1882–91), can be had on application to the Secretary, or to Messrs. E. and F. N. Spon, 125, Strand, W.C. Price Two Shillings and Sixpence each.

INDEX TO VOL. XXIV.

1895.

ABSTRACTS.